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Oral History Interviews
Medical Physics Series

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James Born
Patricia W. Durbin
John Gofman
Alexander Grendon
Thomas Hayes
John H. Lawrence
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William G. Myers
Alexander V. Nichols
Kenneth G. Scott
William Siri
Cornelius Tobias

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History of Science and Technology Program

ALEXANDER GRENDON:
RESEARCH WITH HARDIN JONES AT DONNER LABORATORY, 1957-1978

An Interview Conducted by Sally Smith Hughes

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Alexander Grendon (1967)

Contents

Acknowledgment	ii
Introduction	iii
Curriculum Vitae (Grendon)	vi
Initial Association with Donner Laboratory	1
State Atomic Energy Coordinator	3
Return to Donner Laboratory	5
Jones's Thesis Research and the Founding of Donner Lab	10
The Aeromedical Unit	11
Lipoprotein Research with John Gofman	13
Disagreements about Radiation Effects	21
Radiation Safety Standards	34
The Fallout Issue	39
The Berkeley Student Revolt of the 1960s	43
Jones's Work on Drug Use and Abuse	52
Research on Cancer Therapy Effectiveness	57
Jones as a Scientist and as a Person	60
Jones's Relationship with John Lawrence	70
Debate with Edward Teller	72
The Institute of Human Development	76
Donner Laboratory as an Institution	79
Index	87
Curriculum Vitae (Hughes)	90

Acknowledgment

This interview with Colonel Alexander Grendon is one of several dealing with the development of Crocker and Donner laboratories, within the larger series of oral histories produced by the History of Science and Technology Program of The Bancroft Library.

Besides these interviews, the Program assembles other primary source materials, including the papers and personal memorabilia of scientists and engineers, and the papers of certain organizations with which they were associated. The information in the papers and interviews helps to demonstrate the development of science and technology not only in the western United States, but also in the nation as a whole.

The project was made possible initially by the generosity of William R. Hewlett and David Packard. Mrs. Calvin K. Townsend established the Doreen and Calvin K. Townsend Fund to provide ongoing support of the Program. The University Endowment Fund, National Science Foundation, and National Endowment for the Humanities have assisted diverse aspects of the Program with a series of grants. Further aid has come from the Marco Francis Hellman Fund, established to document science and technology and their relations to business in California. Other donors have included the Woodheath Foundation, the California Alumni Foundation, and the Watkins-Johnson Company.

James D. Hart
Director
The Bancroft Library

Introduction

This transcript is based on an interview with Colonel Alexander Grendon on November 30, 1979, at his home in Sacramento, California, conducted by Sally Smith Hughes. The interview deals with Col. Grendon's association with Donner Laboratory, in particular his research activities with Hardin Jones, associate director of the Laboratory.

This interview is part of a series recorded during 1979 and 1980 with individuals associated with Donner and Crocker laboratories and, in some cases, the Division of Medical Physics and Biophysics. Donner Laboratory is part of the Biomedical Division of what since 1971 has been called the Lawrence Berkeley Laboratory. Crocker Laboratory no longer exists. The Division of Medical Physics, which in March 1980 became a department, is an academic unit of the University of California.

The medical physics interview series is part of a project funded by the National Endowment for the Humanities to record and document the history of the Donner and Crocker Laboratories, the Division of Medical Physics and Biophysics, and the careers and research of individuals associated with them. In addition to the interviews, documents in the form of correspondence, laboratory notes, reports, and so on were solicited for deposit in the History of Science and Technology Program archives at The Bancroft Library. These materials should be useful in constructing a picture of the growth and development of the fields of medical physics and biophysics, in which the Berkeley research and academic institutions played an early and significant role. They also complement other interviews and documents in the Bancroft collection concerning the overall development of the Lawrence Berkeley Laboratory and its research programs and staff.

Oral history can frequently provide useful information on subjects not easily retrieved from published sources. Hence the questions often dwell on family history, social and economic conditions affecting research, interactions with colleagues, and peculiarities of institutional organization. It should be borne in mind that the interview records what the subject remembers during the interview about what happened at a given place and time. In general, information obtained in response to broader questions is more likely to be accurate than answers on specific events, influences, and accomplishments.

The verbatim transcript of this interview was checked against the tape recordings and edited for punctuation, paragraphing, spelling, verification of proper names, and repetition. Col. Grendon reviewed the edited transcript and made minor corrections, additions, and deletions. The index was prepared by Jennifer Snodgrass, who also aided in the final production of the transcript.

Literary rights for this interview are vested in the Director of The Bancroft Library. Any quotation for publication of the material included herein requires the advance written approval of the Director. A request to see the transcript constitutes an agreement to abide by this restriction.

The draft transcript of this interview, as well as biographical data and other background materials, can be found in the papers of the History of Science and Technology Program in The Bancroft Library, and the tape recordings in the Microforms

Division. Users wishing to order these materials, or those pertaining to other interviews in the History of Science and Technology series, may do so through the Heller Reading Room.

Sally Smith Hughes
Robin E. Rider
History of Science and
Technology Program
The Bancroft Library
February 1, 1985

Curriculum Vitae
Alexander Grendon

1899	New York, N.Y., born September 1 Father: George S. Grendon Mother: Natalie Sieger Grendon
1919	B.S., Columbia University
1920-22	Assistant instructor of physics, Columbia University
1922	M.E., Columbia University
1922-40	Engineer for various projects
1940-54	U.S. Army; specialist in radar, radiological warfare, and defense
1948-51	Radiological defense engineer training, Naval Postgraduate School, Annapolis, Maryland, and University of California, Berkeley
1951	M.A., University of California (bioradiology)
1954	Retirement from Army as Colonel, Deputy Commander, Research and Engineering Command, Army Chemical Corps
1957-59	Research assistant in biophysics, Donner Laboratory, University of California, Berkeley
1959-63	Coordinator of Atomic Energy Development and Radiation Protection, State of California

1963-67 Biophysicist, Donner Laboratory

1963-68 Member, National Advisory Council on Radiation

1964-73 Member, California Radiological Health Advisory Committee

1967-78 Consultant biophysicist, Donner Laboratory

1968-72 Member, Radiological Health Sciences Training Committee, U.S. Public Health Service

1978-80 Consultant, California Office of Emergency Services

1978-82 Consultant, Advisory Committee on Reactor Safeguards, Nuclear Regulatory Commission

Initial Association with Donner Laboratory

Hughes: It's November 30, 1979, and we're in the living room of Colonel Alexander Grendon. Colonel Grendon, could you tell me when you first became associated with Donner Laboratory, and why that occurred?

Grendon: Yes, I can. I'll have to give you a little background before my strict association with the Laboratory. The military departments of the United States, the Department of Defense and associates, decided that following World War II they needed some people trained as what they called radiological defense engineers. So they set up a joint program administered by the Navy but including Army, Navy, Air Force, Marine Corps, Coast Guard, Public Health Service, from each of which they selected some officers of backgrounds in scientific areas to take a three-year program to become these radiological defense engineers. The three-year program consisted of a year at the Naval Postgraduate School, and then two years principally at the University of California, Berkeley. Some went to Ohio State, but most went to the University of California.

I was in the group that took that program and went to the University of California, where Hardin Jones and Cornelius Tobias were my professors, among others. Much of the work was done at Donner Laboratory, though we had to take courses in other areas of the campus. That was in 1949 to '51. We

got degrees in bioradiology as a result of our program. Then each of us went back to military service, the service we came from.

When I reached their mandatory retirement age in 1954 and left [the service], I ventured into certain other areas but eventually decided to go back to the university and get a junior college teaching credential, which I did. At its conclusion the department on campus suggested getting references. Since I'd been in the military service, the only references I could get were from professors I'd known on the campus there. When I asked Hardin Jones for a reference, he said, "I'd be glad to give you one, but have you thought of going into research?" I said I doubted that people would start me in at research at my more advanced age, and he said, "You're the man I've been looking for." So he called me in and showed me what they were doing. I said, "Well, I've just got my junior college teaching credential." And he said, "Well, you won't start teaching till the fall, will you?" I said, "Probably not; contracts are generally let that way." "So there, why don't you try it for a few months, and if you don't like it then you can go ahead and do your junior college work, and if you do like it, perhaps we can persuade you to stay here."

Hughes: Why do you think he singled you out for research?

Grendon: Well, because I was obviously the best student in that whole group. Cornelius Tobias, for example, when he sent back the grade card that I'd asked for, wrote "A plus," "outstanding," and things of this kind. In fact, when I returned to the Laboratory, Tobias tried to persuade me to work

with him rather than with Hardin Jones. But it was Hardin who had asked me to, and I'd agreed to, and so I felt committed there. So I went to work with him for those few months and we found that we complemented each other so well that we decided that this was a good working relationship, and I remained. That was probably the beginning of February of '57.

State Atomic Energy Coordinator

I remained, as I say, for a time, because in the summer of '59 the senior Governor Brown, the father of the present governor, had had a statute enacted that set up a post to which the legislature gave the cumbersome title "Coordinator of Atomic Energy Development and Radiation Protection." I had to practice saying that in one breath.

Let me comment first that several other states had set up similar positions because the Council of State Governments had recommended it, but in each instance the governor in that particular state chose either a politician or a lawyer or a budget man or somebody of that type. Only Brown had the good sense to have his aides look in the scientific community. They collected a set of names which one of the aides sent to Hardin Jones in a letter, asking his opinion on these, and saying if he knew of any others who were to be included to please let them know. So he showed me this list of names, this letter, and said, "Now I'm faced with a dilemma. I think you're the best qualified to take this post, but I don't want you to leave me, so I've got to leave it up to you." So I considered the matter....

Hughes: Was your name on the list?

Grendon: No, no. He wanted to add it. He felt that my qualifications were superior to those of anybody on the list, for various reasons. So I thought it over and said to myself, well, it's a clean slate. Nobody's done this, and the state radiological health program doesn't exist, but should exist. So I said, well, add my name to the list. The chances are they won't take me because I've never been involved in politics and don't have much regard for politicians. Several of these aides screened me, and finally one of them said, "We've set up an appointment for you to meet the governor in Sacramento." Hardin said, "What do you think your chances are now?" I'd said about one in a hundred before. I said, well, I don't suppose they've asked more than three people to go there, so I'd say now maybe it's one in three. As a matter of fact, they hadn't asked anybody else to go there, and this was really the final selection, you see. So I went to Sacramento and met the governor. I'd give you a longer story about that but it's not pertinent to Donner Lab's history. I was appointed to that post. Hardin asked how long I thought I'd be in Sacramento. I said maybe half a day. Politicians and I may not get together that well.

But it turned out that I could manage myself quite well with politicians and I did have in mind a program. I said, well, maybe a year to get this set up. You don't do it that fast, I found, so I spent four years up here, in Sacramento, that is. Hardin had asked several times whether I was coming back to the Laboratory or not. I said, "Well, I intend to, it's just a question of getting this set." But finally I announced that I was going to

leave the governor's staff and return to the Laboratory, which I did, after that four-year gap.

Hughes: What year would that be now?

Grendon: Well, that would be 1963 then. I was on the governor's staff from September of '59 to September of '63. The governor made me promise to get him a qualified successor for my post, and I did that.

Return to Donner Lab

So I returned to Donner Lab then in 1963. I stayed with that until I reached the university's mandatory retirement age in 1967. Hardin Jones and John Lawrence tried very diligently to arrange some kind of contract for me to be a consultant, but I'm afraid the problem was that I'd been very outspoken against what was going on during the revolutionary times on the Berkeley campus in the sixties, and that didn't win much favor. So those in authority wouldn't do a thing for me. Hardin seemed very disappointed. I said, well, I don't really have to go to work for a living. If the university will pay my expenses, I'll donate my time. So we started that practice. They agreed to offset my expenses, and I stayed on for eleven more years.

Hughes: What had your title been before you retired?

Grendon: I was classed as a biophysicist. I had a P-grade of some kind, P-numbers meaning professional, I believe.

Hughes: What had you started doing when you first arrived in Donner Lab before your spell in Sacramento?

Grendon: Well, I'll tell you the amusing first task. I came in on a Monday morning, I believe it was, and Hardin Jones was out of town. His secretary said, "He left this transcript for you." It was a transcript of his remarks at some meeting, some symposium. I said, "What am I supposed to do with it?" She said, "He didn't say." I started to read it and said, well, it's either garbled or he didn't say what he meant in places. It's obvious that this must mean something else here. So I put down pages of notes and what I thought might be changed. But I didn't want to tamper with the transcript, because I didn't know what the purpose was. When he returned I told him my puzzlement and what I'd done and showed him these pages and he started reading through them. He read through about one page and said, "Oh, this is great. You've got carte blanche. Change it any way you want." I thought, this is rather free and easy. That was my first task with him.

We shortly got involved in some problems connected with the radioactive fallout in nuclear weapons tests. It was just about that time that the Lawrence Berkeley Lab--well, at that time it was the Lawrence Radiation Lab, or I guess it was called the University of California Radiation Lab during that time; I don't think it had yet been named for Lawrence--it was at that time that some of the people there first ran into the problems of noticing a high radiation background, and they didn't know what it was due to. It wasn't until some observations, outside and inside, that they discovered that it was due to fallout from weapons

tests. So we undertook to make some measurements and estimates of what effects this might have. Margaret White--do you have her name down anywhere?

Hughes: Yes, I've come across that name.

Grendon: Margaret White had long worked as a technician for Dr. Jones, had done the laboratory work that was associated with experiments. And Margaret did the laboratory work here too, the measurements that were needed. Hardin and I and Margaret jointly wrote up a paper on our observations and the probable effects of the radiation levels we reported. That was published, I think, as a UCRL report. Then from that we advanced into experiments on radiation effects, and we got involved therefore in some of the comparable phenomena, like aging.

We worked on some studies of aging effects in general, using data from--not data that we obtained directly, but data that had been in the literature from various sources, to try and synthesize it into some kind of a theory as to what was going on in the aging process.

Hughes: At what stage was Dr. Jones's theory of aging at that time? Was it fairly well formulated?

Grendon: Well, the first paper that he wrote on the subject was a paper he presented to the Health Physics Society before the Health Physics Society had a journal. So it was published in what was called the Proceeedings of the Health Physics Society. There was only one issue of the Proceedings, and I don't think it was ever published in book form. I

think it was just a mimeograph sort of thing. That was in 1956, when he'd been invited to speak to them. He'd suggested that you could draw an analogy between physiological age and chronological age and the effects of various factors on physiological age among individuals; that is, what effects you could get from exercise, from smoking, from overeating or undereating, being overweight or underweight--in other words, various factors that might influence it. He began to work on that about that time and presented some of it in that first paper.

Hughes: Do you know what started him thinking along these lines?

Grendon: Yes, I think primarily it was that his background was in physiology, you understand. At that time, most of those who were professionally involved in [work with radiation effects] felt that there was some nonspecific aging effect, so to speak, in radiation injury. Right now we think that the production of cancer is the one risk clearly associated with radiation, and many say that there is no other factor than that involved in the radiation risk.

But not everybody says that even now. Some still say there is a generalized aging effect, and there certainly seemed to be. It seemed that if you had a group of irradiated animals and a control group and compared them, that, aside from cancers, those who died of nephritis or pneumonia, or anything else, died earlier in the irradiated group. So they said, well, this in effect is an aging process. They're the same chronological age, but physiologically they act like older animals.

Now, there are also some apparent symptoms that really are rather tricky, like the graying of hair, but that can be radiation of hair follicles specifically, and may not signify what is happening to the organism as a whole. But [there were] various factors that made people say there's an aging effect of radiation, and so it was that that got him to look at aging in general, I believe.

Hughes: When did he become concerned with radiation effects per se? I know he started off in something entirely different.

Grendon: Well, he used radiation as a tracer in his early work on a Ph.D. thesis, for example. He used radioactive phosphorus as a tracer. And so the use of radioactive materials for tracing processes led to a concern as to what radiation might be doing in the way of damage to the organism. He was one of the early ones to call attention to this risk.

He testified in some hearings before Congress along about that same time, 1957 or so I think it was. In hearings before the Joint Committee on Atomic Energy he claimed that there was this aging effect of radiation--that is, not really specifically cancer. As I say, nowadays most feel that really the added deaths you get, in other words, the average aging of a group in terms of its mortality, is only due to those that die of cancer. Many contend that now, but it seemed then that there was this generalized aging effect, and so he began to look into the process of aging.

Hughes: Which led him led him in many directions...

Grendon: Yes. He was chosen to be the scientific coordinator, I guess it's called, for the White House conference on aging, about 1957, '58. I'm not sure what year it was.

Jones's Thesis Research and the Founding of Donner Lab

Hughes: Well, now that we're onto Dr. Jones maybe we could backtrack for just a minute. I know it was before your acquaintance with him, but do you know when he first came to Berkeley in the late thirties? I know he was interested in plant physiology. Do you know how he made the transition to [I.L.] Chaikoff and the use of tracers, and from there to John Lawrence?

Grendon: Well, Chaikoff was his research advisor for his thesis. He worked as a graduate research assistant for Chaikoff.

Hughes: I thought he had briefly been in a different field before switching.

Grendon: Well, he'd been in a couple of different fields. He'd started a pre-med course as a matter of fact. He'd thought of becoming a physician, and then decided that physiology interested him more, and so he switched back to physiology.

Hughes: This would have been during the war?

Grendon: No, during the thirties. By the time of World War II he was well entrenched in the Donner

Laboratory. Did you ever hear any account of how Donner Laboratory itself originated?

Hughes: Mr. Donner and the Donner Foundation, is that what you mean?

Grendon: Well, this is a secondhand tale. Hardin Jones told me. There are several aspects. First, that Ernest Lawrence, who was the physicist, persuaded his brother John, who was at Yale at the time, [that] there [were] opportunities to use these radioactive materials in medicine and [that he] ought to come out here and start doing some research in that, and persuaded him to come out. They'd started to do some work, I believe, [when], according to Hardin's account, one day a man unknown to Ernest Lawrence came in and talked to him about what they were doing in this [field] and so on and pulled out a check for $100,000 and said, "I represent the Donner Foundation. We'd like to support this work, okay?" That's the story he gave me.

Hughes: If it could only be that easy!

Grendon: Yes, nowadays it isn't quite that easy. Now you have to fill out reams of forms if you want to get a grant of any kind.

The Aeromedical Unit

Hughes: I know that Dr. Jones became associated with the aeromedical unit, which was actually the function of Donner Lab during World War II. Is that true?

Grendon: Well, it was one function. The Navy was concerned with the effects of diving, the Army Air Corps was

concerned with the effects of high altitudes and changes of pressure, and so he did a good deal of research on respiration under these conditions of extreme changes of pressure. He used radioisotope tracers for that. Radioactive xenon, I believe, is what he used to measure effects there, because you can breathe it in and out and get measurements of volumes and rates and so on. [He] published a number of papers, not for general publication, not in the journals--not classified, but they were submitted to the military services for their use.

Hughes: I know they had the use of a bomber at some stage during their war research. Do you know how those arrangements were made?

Grendon: Well, they were doing this work under contract to military services, so no problem. I mean, if they needed a bomber, they'd get a bomber, of course. I don't mean just given to them to play with. If they needed to set up an experiment that required flying up to some high altitude, they'd ask for the equipment to fly, I'm sure. I have never discussed that with them, but to me that seems rather obvious that it would be the way they would do it, because it was under contract.

Hughes: Do you think there was any sense of disappointment when at the end of the war pressurized planes came into use, thereby making less important a lot of the research that had been done in the aeromedical unit?

Grendon: Oh no, I don't think that would have bothered them one bit. The scientific viewpoint doesn't worry about that kind of thing. Applications are really

a secondary thing. And they learned a lot about what happens in terms of human respiration under unusual conditions. They never stopped that kind of research. Will Siri, for example, who is a mountaineer--he was in that Everest climbing group --for a number of years continued experiments on respiration at high altitudes and such, which I think was largely inspired by the earlier work on these respiratory conditions that Hardin worked on.

Lipoprotein Research with John Gofman

Hughes: After the war, I believe in 1945 or 1946, John Gofman appears on the scene.

Grendon: Yes. John Gofman had been on the scene before that. Hardin, John Gofman, Frank Lindgren, several others got interested in the lipoproteins in the blood, the combination of fat and protein in the structures, and began studies. Hardin was in a rather supervisory capacity over it. One of his talents was what you might call scientific intuition. Another of his talents was laboratory skill. I mean his dexterity and skill in doing things, ingenuity in devising ways to do things, was very remarkable.

Hughes: You're speaking now in a mechanical sense, or actually putting together an experiment?

Grendon: Both ways. But I think a mechanical.... Well, for example, he had a remarkable, extensive collection of butterflies and moths, and to watch him repair the wings of a butterfly or moth specimen with such delicacy of touch--really fantastic. I brought him those specimens to add to his collection

[indicating specimen case] but it turned out that he had some, so he returned them to me, mounted in that little case there.

Hughes: Isn't that beautiful! It's a work of art, isn't it?

Grendon: Yes.

Hughes: When you say scientific ingenuity...

Grendon: Intuition is what I said. He just had the right hunch as to which way to look.

Hughes: Is that something that somebody's born with?

Grendon: No, I don't think it's that. I think it's a combination of things. It's a combination of having imagination and storing up in your mind a lot of background so that it kind of points there and you say it must be this way, even though you don't have any proof that you can put in an article and publish in a journal. But you say, this is really the direction that must go, and this must be the cause of that. And then you start doing experiments [to] find out whether it is the cause, and often you find it is.

So on these lipoproteins... The material that is found in hardened arteries, that tends to build up and block them, has long been known to contain, among other things, cholesterol, but it also contains materials of the lipoprotein category, all built into a mass that tends to obstruct blood flow. And so they decided to try to see what relation there might be between the lipoproteins and atherosclerosis. I think it was John Gofman's

idea, though I'm not sure whether I'm crediting the right person, to use the ultracentrifuge that had just recently been invented--this extremely high-speed centrifuge with a viewing device that lets you view it while spinning. That is, not truly view it, but view it through its effect of producing a curve by means of the light passing through it. [They used] this to distinguish the kinds of lipoproteins, in terms of density. The more dense would move out faster than the less dense. And so they decided to look at the behavior of several different groups [of lipoproteins].

Over the years they changed their grouping. First they decided that this group was bounded by this rate of flow, and this group by that. Later they moved the boundaries around, and so on. They had discovered that people who had atherosclerosis showed certain patterns of these lipoproteins in their blood and therefore they felt that this might be a predictive thing, that if you found somebody who showed no signs of having atherosclerosis but who showed these patterns of lipoprotein structure in the blood, you could say [they were] likely to get atherosclerosis. Because cholesterol seemed to be one of the factors, they at first were advising people to hold back on cholesterol in the diet. Later they decided that the body manufactured enough cholesterol, out of saturated fats particularly, and so they emphasized saturated fats even more.

I was one of the guinea pigs in the early stage. It was in 1949, when I first came to the university--well, I guess it was in our first year when a man that I didn't know came to Tobias's lab when we were taking one of our experimental courses. He said, "Is there anybody here over the

age of forty?" and they all pointed a finger at me, because I was, I guess, the only one. So he came over to me and said, "I'm John Gofman. We're doing experiments of this kind, and for our experimental purposes we need blood samples from men over the age of forty. We can't pay anything for it, but if you would agree to work with us I could promise to give you an 'A' in any course you took." They all shouted, "He gets 'A's!" So I said, "Well, I don't need to be bribed. If this is useful to your experimental studies, yes, you may take blood samples." So during the two years that I was at the university I gave them blood samples periodically.

After I'd given them the first couple of samples... This has nothing to do with Donner Lab. I don't want to take your time telling about it.

Hughes: Go ahead.

Grendon: It was just at New Year's and I was the senior officer of this group and so my wife and I invited all the others for a New Year's Eve party at our home. It was that evening that Hardin Jones phoned and said, "Your blood shows the pattern that we predict leads to atherosclerosis and you should promptly go on a low-fat, low-cholesterol diet." I said, "When you say 'promptly,' how promptly do you mean?" He said, "Immediately!" I thought to myself, this is ridiculous, but here it is. My wife had prepared all these delicacies, and I said, well, here's a test. So I didn't eat any of them and everybody ridiculed me. I thought, well, if you're going to find out, you'd better find out whether you can make it the hard way.

An interesting sidelight on it, too: Some years after I returned to my military duty and then came back to the university after retirement, [I] looked up Arthur Tamplin, who worked with Gofman as his technician primarily, to discuss this. I asked, "Anything new about it?" "Well, we've changed our ratings scheme." "Oh, have you? That presumably would affect what you might have said about somebody." He said yes, it did. I said, "How would it have affected mine?" He looked up the records and said, "Well, you would not have been classed as a positive." I said, "Now you tell me!" Oh, dear.

Hughes: After five years of dieting!

Grendon: Yes.

Hughes: I was reading an account written in 1969 by Dr. Jones about Gofman and this lipoprotein work, and he mentions that after the studies had gotten to a nationwide stage, there were several labs, I believe four in number, which were pooling data. A controversy apparently arose about whether the cholesterol or the lipoprotein component was the better index of cardiovascular disease. Apparently the data collected by the Donner group pointed heavily to lipoproteins being the better index, and there was another lab that said the cholesterol seemed to be the better index, and I think the other two labs stood behind Donner Lab. I understand that Dr. Gofman received some criticism at that stage. This is only through [the correspondence of] Dr. Jones that I know about this. Apparently Gofman was accused of not handling the cholesterol data as precisely as he

handled the lipoprotein. Do you know anything about that controversy?

Grendon: I do know about the controversy. I also know that for years before that, people had sought to associate cholesterol with atherosclerosis but found the association just didn't work, that apparently people who didn't take in much cholesterol in the diet nevertheless may have had high cholesterol levels because they manufactured a lot of it. And so cholesterol in the diet didn't seem to be an important factor there. They eventually came to place more emphasis on saturated fats than on cholesterol.

At the time of their conclusion that lipoproteins were a factor, Dr. Gofman's wife, Dr. Helen Gofman; Dr. Jones's wife, Helen Jones; and three others, dieticians E. Virginia Dobbin and Clara-Beth Young, and Lenore Lyon, prepared this low-fat, low-cholesterol cookbook, which had an introduction, I think, by Dr. Thomas Lyon and Gofman. For twenty years it continued to sell. Twenty years later, when the publisher was running out of copies, he asked the ladies if they wanted to revise it or whether he should just reprint it, and they said there had been many changes. So they revised it and I was asked to edit that, as a matter of fact, and so I did edit the second edition.

In the second edition, throughout the text --the publisher insisted on the old title--they added the word "saturated" to the words "low-fat," because low-fat didn't seem to be it. If you had highly unsaturated fats, no problem; in fact it might even have improved the situation. It at least presented no problems, and may actually have

been an advantageous factor, over not having any fats at all, let's say. So they included the words "saturated fat" in their low-saturated-fat, low-cholesterol diet.

There was certainly a controversy and it still persists about whether the lipoprotein structure or some other factor is really at work in it. It was agreed by all, I think, that high blood pressure precipitated some of the results, no matter what the diet consideration--that high blood pressure was certainly an initiating factor. But you see, many people have high blood pressure, and if you can control the outcome, control the occurrence of atherosclerosis by modification of the diet, of course that has value.

Hughes: What was the breakdown of labor in these lipoprotein studies?

Grendon: Well, the laboratory work was largely done by Frank Lindgren. There may have been some others but I know that Frank did do a great deal of actually operating the centrifuge. Later on they used other methods such as gas chromatography and things of this kind.

Hughes: I've seen Lindgren credited with the discovery of lipoproteins.

Grendon: No, I don't think that's true. I think that lipoproteins were known. The group really took credit. The senior authors would vary from paper to paper and so you couldn't really say who initiated it, but Frank was in on the early work.

Hughes: What was Hardin Jones's role?

Grendon: Well, he was really sort of a senior advisor, because of his extensive knowledge of physiology and both his scientific intuition and his skill in handling experimental material. He would be in on these discussions and help to direct the way they were looking. But, specifically, I can't pin it down any more closely than that.

Hughes: And John Gofman? He was taking care of the medical aspects?

Grendon: Well, yes, that largely. Also I think he was the one who first suggested the use of the ultracentrifuge--these little charts, these little diagrams that came out of it--and interpreting them and trying to suggest breakdowns of the patterns; that is, what grouping might be important.

Hughes: And all these data were gathered through Cowell Hospital?

Grendon: No, I don't even think I went to Cowell Hospital to give the samples. I'm not sure where they took samples. The Laboratory had its own medical department, that is, for taking care of patients who came there as subjects to be studied or assisted, and I think it was right there in the Laboratory that I gave these samples.

Hughes: I know there was a diet table for a while in Cowell Hospital that Ernest Lawrence, for example, sat at....

Grendon: I never was in on it.

Hughes: Maybe that was earlier on.

Grendon: Well, it was pretty early that I got involved in it--that is to say, it was 1949, I think.

Disagreements about Radiation Effects

Hughes: In the late fifties, the association seems to have broken up. Gofman goes one way and Jones goes another way. Was there any precipitating cause?

Grendon: Yes. It was in the sixties that things began to get difficult, and that was over other matters. John Gofman had taken a similar position to Hardin Jones about radiation, that yes, there's some risk of harm from exposure to radiation, but that on balance many of the applications of it are certainly worth doing, whether it be diagnostic X-ray or use of radioactive tracers in people or as an efficient process to produce energy and have radioactive isotopes as a byproduct.

The reason I say it was in the sixties is that the revolutionary movement on the Berkeley campus began in the fall of '64, and it was shortly after that that I recall that John Gofman began to take an anti-establishment posture. He was against the establishment in general, and he was against nuclear power therefore, and he began to have different viewpoints about this particular matter. Dr. Gofman had a balanced view before; now he was denouncing them, making statements that were at variance with what he had said before. [I] questioned what his motives were, of course.

Hughes: Do you have any ideas about motives?

Grendon: How do you tell what a person's motives are? I have some ideas, but I don't know that I should put them on the record.

Hughes: So it was a philosophical parting of the ways rather than a scientific one, although of course the one precipitated the other?

Grendon: Yes. The only area where there was scientific disagreement was as to the effects of radiation. John Gofman began making extreme statements about all the dangers of radiation from the nuclear energy programs of the country. He was predicting enormous numbers of deaths of people from cancer, and so on. He still is doing that. He still is one of the figures called upon by the antinuclear people all the time as their scientific backup--he and Ernest Sternglass from Pittsburgh and a few others who are their scientific foundation. Most of the antinuclear people of course don't profess themselves to be scientists, but when they need scientific backing they run to Gofman or some of the others.

Hughes: What was the scientific basis for Gofman's beliefs about the dangers of radiation?

Grendon: Well, it depends on how you want to work it.

Hughes: As I understand it, the main burden of his argument is the danger of low-level radiation, that the safety standards are set too high.

Grendon: Yes. You've talked to Dr. Gofman?

Hughes: No, I haven't, although I'm hoping to.

Grendon: Oh, he'll be glad to see you. He's a very willing spokesman. He and I have debated several times under various conditions. You see, his emotional involvement tricks him. The emotional involvement of the scientists who are antinuclear is unbelievable. Let me give you some illustrations of John Gofman, and afterward, if I may, of this Dr. Sternglass. It has nothing to do with the University of California, but it's an interesting point.

Dr. Gofman and I were scheduled to debate near Santa Cruz--Davenport is the place. PG&E was considering putting in a nuclear reactor there. The supervisors of Santa Cruz County, I guess it is, had asked Dr. Gofman to address them and they'd asked the Laboratory to suggest somebody who would speak on the other side, and I turned out to be it. Shortly before that, at the Joint Committee on Atomic Energy hearing in Washington, Dr. Gofman had said things, as he had on previous occasions, about Dr. Bond from Brookhaven National Laboratory. Victor Bond used to be a Donner Lab man. Gofman also said things about me, and they got in the [Congressional] Record. He was printed in the record using very vituperative terms--corrupt, dishonest, and all sorts of things.

So a day or two before our debate, we met in the hall of Donner Lab and he invited me into his office, presumably to talk about our forthcoming debate. We got to talking about these others and he began to get more and more excited about this. So I said, "Jack, why must you call Vic Bond these

names? What good is that?" "Well, when he attacks me, I can attack him." I said, "He has never attacked you. He has said you were mistaken about something scientific. And what do you do?" He said, "What am I supposed to do?" I said, "What you're supposed to do is say, 'No, I'm not mistaken. Here are the facts on which I rely and here's the argument. Here's why I think I am right.' Calling him names doesn't convince anybody." I didn't mention that he'd also called me names.

But the point is that here's a man who is brilliant--because I think he is. He certainly has scientific knowledge, but his response is to call names. And you say [to yourself], that isn't reasonable. There's something wrong about the situation.

Hughes: Is this something that arose in connection with the nuclear problem?

Grendon: No, I think it arose in connection with the revolution on the Berkeley campus. Let me comment there.

When a number of students were arrested in a sit-in on the Berkeley campus in the earliest days of that revolutionary thing, two of them happened to be graduate students in Donner Laboratory. Now, Donner Laboratory had something like a hundred or more graduate students at that time, I think. Two students in that hundred or more turned up with beards. Which two? The two that were arrested, because at that time that was a symbol of defiance. Shortly [afterward], John Gofman turned up with a beard. So did Arthur Tamplin, who was working closely with him. It wasn't that they thought a

beard made them look more distinguished or anything; it was flying a flag and saying "I'm against the establishment. If the establishment is smooth-shaven, I'll wear a beard." And so this anti-establishment thing led him to all sorts of distorted views.

I audited a course he gave about the induction of cancer by radiation and the carcinogenic effects. He sat up on a desk here, addressing the class and saying of all the formal bodies, the National Council on Radiation Protection and Measurements, all these bodies, they're corrupt, they're all dishonest, they're all a bunch of liars. This is ridiculous. You're talking about the best-known people in the field. If you think they're mistaken, that's one thing, but they're all corrupt?

I started to mention about this debate that we were going to have in Santa Cruz and how we happened to talk about Victor Bond. But there's a sequel to that about the debate itself. Shortly before that occasion, Dr. Leonard Sagan from down in Palo Alto had drafted a statement about nuclear energy that he invited a number of scientists to sign. He invited me too, and I did sign it because I agreed with it, and it turned out there were 29 of us who had our names on this.

When Gofman and I were debating before the supervisors at Santa Cruz, I recognized that one of them had been tipped off with questions by somebody who knew something about it. [The supervisor] obviously didn't know anything about it, but somebody had fed him questions. They were questioning Gofman and me in turn, [and] at one point this supervisor said, "I'd like to ask Dr. Gofman, are you familiar with this

letter signed by twenty-nine people?" Gofman said, "I certainly am. May I have it?" And he took it, and he said, "So and so, fully supported by the AEC; so and so, works for the AEC; so and so, funded completely by the AEC; so and so, receives support from the AEC. Grendon? Well, he doesn't." He couldn't attack me in any way because there was no tie there, so he skipped over my name.

But he went on to several and handed it back, and this supervisor said, "And now I'd like to ask Mr. Grendon this question." I said, "First let me comment on that letter." So he turned to the chairman and said, "Mr. Chairman, I ask that the speakers be directed to answer these questions in turn." I said, "That's what I'm trying to do. I'm trying to answer your previous question before I go on to the next." I said, "I think it's outrageous for Dr. Gofman to impugn these people. Here, for example, Dr. Alexander Gottschalk, a physician heading the cancer institute of Argonne National Laboratory. Do you think he needs the AEC salary to earn a living? Do you think he can't earn much more outside? How do you think this then buys him and corrupts him?" I said, "This is outrageous, to call these people corrupt because of any funding they may get from the AEC." But, you see, that emotional involvement kept [Gofman] from approaching it the way one would expect a scientist to approach it.

I spoke about Dr. Ernest Sternglass, from Pittsburgh, and let me tell you something of that. When he was scheduled to address the Health Physics Society in New York, I think it was in 1971, the chief of the Bureau of Radiological Health in California called me up and said, "Are you going to that meeting?" I said I was. He

said, "Well, none of our people can because we don't have any funds for travel." But he said, "We gave Sternglass some data and he's misusing it. We saw a draft of his paper, and we'd like somebody to correct it." I said, "I'll be glad to." So I got the data and I had a slide made up.

Dr. Eugene Saenger, from the University of Cincinnati, was chairman of that session. I said, "Gene, when the session is prepared for discussion, please call on me first because I want the projectionist to show a slide, and he usually leaves when the speaker is through." So he did, and I had given this slide to the projectionist. I said, "Dr. Sternglass told you that infant and fetal deaths increase as radioactive effluents from the nuclear reactors increase. As one example of it he showed you the situation at Humboldt Bay on the north coast of California. He showed you two years in which the effluents rose from the one year to the other, and the infant and fetal deaths rose from the one year to the other. What he didn't show you is what the Department of Health gave him, which was six years' data. In each year the effluent increased, right straight through the sixth year. In each year the infant and fetal deaths decreased right straight through except one year where there was a measles epidemic throughout the state. He picked that year and showed you that to try to persuade you that the radioactive effluents had done it." Now this in effect is calling him a liar.

Sternglass made some doubletalk response. Later in the meeting--it was a four-day meeting--he ran into me and said, "Are you going to be at the meeting in Berkeley next week?" I said, "Why, of course; that's my own stomping ground." So he now

knew I was going to be there. He was on the program. He presented the same material in the same way. I wasn't prepared for that, though I did have the slide with me, so I waved a hand. The chairman didn't recognize me. He said, "The session's over." I said, "Well, I have a slide I'd like to show," and he said, "The session's over." I said, "Well, anybody who wants to stay may see the slide." They were all standing but they all stayed and I showed the slide with the same thing, and made the same remarks.

That evening, Professor Jerzy Neyman had some of us at his home for dinner, and who should sit opposite me at a rather narrow table but Ernest Sternglass. Do you think he was troubled? Affable. Friendly. Nothing disconcerted. And it was twice publicly I'd really said, "You're a liar." But it doesn't bother him, because he doesn't care. He has a cause to fight [for], and that's something like John Gofman. He has a cause to fight. And he doesn't care if you can show that he's wrong. He doesn't do what Sternglass does. Sternglass corrupts the data.

For example, in one of his papers, earlier than that, when he was trying to show the connection between fallout from nuclear weapons tests and infant and fetal deaths, he showed data for New York State from 1935 to 1970 or whatever, plotting each point, showing that there's correlation with the strontium-90 from fallout and the infant and fetal deaths. But there was a gap of about eight years there where he showed no points. I thought, well, surely New York State didn't stop collecting data. So looking it up, I found that he reason he didn't show it is that it diverted completely from where his curve was, and

he didn't want that, so he just didn't show it. This to me is total dishonesty for a scientist. Those who get involved that way forget they are scientists.

Hughes: The issue becomes more important...

Grendon: The issue becomes the one thing.

Hughes: Well, backtrack a little bit and clarify for me, please. You said that Gofman and Hardin Jones--I'm paraphrasing--started out with a similar view about radiation and the dangers thereof. Can you pinpoint for me what their positions were, and what led them to believe...?

Grendon: Well, Hardin I think was first. Gofman followed along. Hardin was the one that alleged and tried to demonstrate that you could predict this increased risk, which he interpreted as an aging phenomenon. Increased risk of mortality he tried to quantify in terms of age--say, here's a group whose increased risk of death makes them subject to the same risk as people five years older than their chronological age, or ten years, or whatever. So he'd say this effect then is equivalent to five years or ten years, or whatever, of aging. In the case of radiation I believe his figure was that five roentgens or thereabouts, meaning roentgens, rems, rads--they're not so far different from each other in general--five r of radiation, let's say, was equivalent to one year of aging, I think was what he had. In other words, a group that had received that much radiation would die at the same rate as people one year older than their chronological age.

Hughes: He arrived at that figure by looking at that data?

Grendon: No, by taking animal data. We couldn't take it with humans because you don't have that much data. Inference from animals led him to that conclusion. Now, using that, he said that this is what you'd expect from it. But also looking at what radiation one would expect from the use of the fission process of nuclear energy, he figured that that would not make any significant difference because it would not add enough radiation to produce much of a change. John Gofman adopted that same viewpoint in some paper, an early paper of his that I recall reading, but later on began to try and find ways to prove that the danger was greater than previously estimated.

Hughes: Had they been working together on this up to that point?

Grendon: No, they had not. They never worked together on the radiation question. But when Gofman began to direct his attacks against the establishment in general, he began to stress these risks and try to demonstrate that they were greater than indicated. Well, no scientist that I know of, other than a few people like Sternglass, went along with his argument, because [it wasn't] that forceful. Some of his claims were really extreme. You'd say, well, if this was really the case, you'd find people dying all over.

Hughes: What was he basing these remarks on?

Grendon: Well, you can make all sorts of analyses of the data which is limited. We don't have a whole lot of data on radiation effects in humans--only at very high levels of radiation, and even those are not too precise. The victims of atomic bombing in Japan: it's very difficult to be precise about what doses they were exposed to, and you get variable results [regarding the carcinogenic effects] depending on what cancers you look at. Leukemia seems to be the only one that really was enhanced. Certainly for many years there was nothing to show that the others really were enhanced at all. You can get some data from groups of people who were therapeutically irradiated, people who had a disease called ankylosing spondylitis, which is a disease that causes painful stiffening of the backbone. Intense radiation was used as a treatment to alleviate that. So here were groups of patients whose radiation exposures supposedly were known better than in the case of the others. But these were people who got thousands of rads of radiation in the course of their treatment, not just a few millirads or so.

The consensus among scientists involved in this area is that if you're trying to interpret effects at low level, where you can't make any measurements, not even on animals, the assumption you make is that the effects are projected linearly down to zero dose. And that assumption is the cautious one to make if you're trying to make rules about radiation exposure. You say, we don't know that it happens, but let's make the rules as if we knew that it happens. [Gofman] insists it does happen, and insists on it to the letter, down

to the most infinitesimal. Now we're all of course exposed to radiation from natural background. He doesn't worry about that; he just directs attention to this radiation effect that he projects there. You get variable numbers as to what number of cases to associate with any given dose, depending on which set of data you work with. Mostly, I'd say, it's done by extrapolation from other animal species. I think Hardin Jones was the first to suggest this: If you look at this aging effect in terms of percent of life span, you come out with fairly consistent numbers for different species even though their life spans are different. But if you look at the aging effect of an adverse condition like radiation in terms of percent of life span, that can sort of unify the data. So you can get variable numbers to work with, and if you want to be as pessimistic as possible, you can get numbers that seem very terrifying.

At a much later stage, 1974 I guess, Hardin and I worked up a paper based on our studies of the carcinogenic effect not only of radiation but of various chemical carcinogens, in which we analyzed it and reported our conclusions that the number of calculated cases of cancer from a given dose is not in itself sufficient to define what the risk is. As you get down to lower and lower doses, whatever cases do appear, appear later and later in life. This we formulated in terms of a mathematical relation, that the latency, the delay, the time lag, was apparently inversely proportionate to the cube root of the dose. So if you reduce the dose by a factor of a thousand, you increase the latency by a factor of ten. Although one might compute numerically that the number of cases of cancer that ought to arise at this extremely low dose is a

thousand, you also could compute that the time in life when they would occur is when you reach age 100. And so you say, okay, so here [are] a thousand cases that really never occur.

This went unchallenged for a long time. Recently there have been some challenges offered to it; I was intending to make a reply to some of these challenges, but I haven't done so. But I'm convinced that this is indeed why we don't see all the cancer cases you would expect to see from environmental contaminants.

For example, in the paper that we jointly prepared a couple of years later than that, we took the vital statistics data that we had for the latest period, which was 1971. We took it for '61 and '51 so as to get two decades, and we took the five-year age brackets in which the data are reported, so that we gave 18 age brackets for these three periods of statistics. What we considered was cancer of the G.I. tract and the liver, because contaminants mostly have to go through the G.I. tract and the liver. It's mostly stuff you eat or drink; even when you breathe it, you're likely to get it down to the gut because it seldom remains in the lung. Everything that goes into the lung and is worked up back out is swallowed again. And the liver is the detoxifying mechanism of the body. So we said, there's where you look for these effects. You say, well, over that 20-year period and before it, that is, from the beginning of World War II, there had been an increase in contaminants. So you'd think that cancers would begin to show up there. But we found that in <u>every</u> instance, if you take the age-specific cancer incidence of the G.I. tract and liver, the digestive system as a whole,

it went down from '51 to '61, it went down in '61 to '71, in each age bracket.

How can you account for that, with increasing contamination, increasing carcinogens around, even increasing nuclear energy use and so on--how can you account for this decrease? The only way we could figure it is exactly what we said: Because we're dealing with these low levels, these cancers aren't appearing in the lifetime of the people. They die from other causes, especially cardiovascular causes. But they die from other causes before these cancers ever show up. I've challenged those people who object to our thesis. I say, "Well, then, you offer some explanation for this fact. We think we have an explanation. If you don't like that explanation, you think we're mistaken about it, what is your explanation of these statistics?" And nobody's offered one. Those who criticize us just simply criticize us and say we're wrong about it, but they don't offer any explanation of the phenomenon that is occurring in the world at large. I believe in looking at what is happening in the world.

Radiation Safety Standards

Hughes: I know that in 1950, Dr. Jones was chairman of the Committee for Consideration of Human Application of Radioactive Materials.

Grendon: And remained that for years.

Hughes: Was that a university committee?

Grendon: Oh, yes.

Hughes: Not strictly at Donner Lab?

Grendon: No. Any user of radioisotopes initially had to be licensed by the AEC. Later, after I had arranged for California to become what is called an "agreement state" to take over many of the functions of the AEC, it had to be licensed by California. But right from the beginning, when AEC licensing was required, the agreement state didn't arise. We didn't become an agreement state until 1962. So prior to that you had to have an AEC license to use these materials.

The AEC wouldn't permit the use of these materials on people in any sense unless the organization that wanted to do it had a human use committee, because they wanted to be sure that the organization was qualified to control this in a way that wouldn't endanger the participants in the work, whether they were patients or subjects of an experiment. It was particularly important for subjects of an experiment, because if you were trying to gain information by using people and exposing them to radiation, you had to be sure they knew what you were doing and why you were doing it, that they weren't being unreasonably exposed, that they knew what risks they were presumably accepting in the process. So this human use committee had to do it.

I on several occasions did some calculations for Hardin Jones for the human use committee as to

what this particular proposed experiment would do, or [I] would review what the experimenter said it would do.

Hughes: Were you in the habit of at times refusing?

Grendon: Oh, the committee turned down various people's requests, yes.

Hughes: Always on the grounds of safety?

Grendon: Well, for example, they might turn it down to this extent: They might say, "You could get this result with one-tenth of that dose. This is a rather high dose to be exposing the subjects to, so you should do it with one-tenth that dose. Here are the results you could get if you used this kind of technique."

Hughes: Presumably the committee was using the AEC standards as a guide?

Grendon: Well, yes. The AEC was using the standards of a different body--it was originally the National Committee on Radiation Protection; then it became the National Council on Radiation Protection and Measurements. Let's call it the NCRP for short. The NCRP developed the standards; the AEC standards were merely a bodily acceptance of this. They just took the NCRP recommendations and made them their standards. So in the one case it was a scientific standard; in the other case it was a legal regulation. But the legal regulation accepted the scientific standards of this other body so they wouldn't have worked merely to a legal standard and said that was okay. But they did accept the work

of the NCRP and its counterpart, the ICRP, which is the International Commission on Radiological Protection. These were almost identical, too, because members of the NCRP sat on the ICRP and were persuasive in getting them to accept the same thing and vice versa.

Hughes: Wasn't there a potential conflict there, in that Hardin Jones was protesting that the levels set by the NCRP were too high?

Grendon: No, I don't think he did protest that they were too high. He drew the attention of the community to the possibility that you had effects at low level that might not have been considered. But the levels they had weren't ones that he really condemned. I believe one of his contributions was to introduce the concept that the then-current idea of a threshold of radiation safety was a faulty one. Hardin, I think, said on occasion that he introduced that idea, but I don't think that was precisely true. There were those who felt that--like with a drug, a chemical agent of any kind--at some point your system could dispose of that drug and [it would] leave no effect. So you found a threshold for it, or at least so it was believed. Nowadays they're not even sure of that in some cases. But at least it was believed that there was a threshold. Below this point you can't find any effect, and therefore you can label it a threshold.

Now, although you couldn't find any effect with radiation below some point, that was rather a statistical and experimental difficulty, and not, indeed, a sure sign that there <u>was</u> no effect. This began to enter into the consideration among many,

Hardin included. He was one of the early ones to speak up about this, I think. I don't know that he was necessarily the first, but he certainly was one of the early ones to stress this point and say you can't make this assumption that there is no effect below some point. All you can do is say, well, we found this effect at every level we could test, and since we can't test it down here, we must merely assume that it keeps going, that since here you have a high-energy action, a violent action on the mechanisms of a cell, all you can say is that it may occur at the very lowest levels. Now, if you make that assumption, then you do indeed oppose anybody who holds to a threshold theory.

But along about that same time, the NCRP itself was denying a threshold theory. The timing is not certain as to who said which first in the mid-fifties. But there was more than one person who began to say we can't really prove or even accept the idea that there is a threshold for radiation effects, and so we must, out of caution, assume it continues right on down. As I mentioned earlier, John Gofman may even say, and he does say, I think, that it's even worse at the low levels. But there's nothing to prove that. We don't know what happens at lower levels, and so there's nothing to prove that it's worse. We think it's bad enough to say it really does continue all the way down to zero, because it may well be that the repair mechanisms of the body can correct all the small effects that occur at very low levels.

Hughes: Were the standards revised at some stage to adjust to this new concept of proportionality?

Grendon: At first the NCRP issued its handbooks through the National Bureau of Standards. They were NBS handbook number so-and-so, which was sponsored by and prepared by the NCRP. In later years the NCRP began to issue its own bulletins under its own numbers--I think they call them bulletins now. But in these NBS handbooks that came out under NCRP auspices, there'd be a little text preceding tables of values. They published an addendum later on, which was to be inserted in one of the earlier handbooks, which discussed this question of threshold and said that there was really no basis for maintaining that there is a threshold. That was somewhere around 1959, I think, that they said this. It wasn't a brand-new idea as of that date. It had been growing and finally they felt it appropriate to add it to the handbook to make sure that nobody misunderstood, and pointed out that they never really had said there was a threshold, and they hadn't.

Hughes: So you don't think that Jones ever argued directly for a lowering of the current standards?

Grendon: No, I don't think he did. He merely tried to quantify what would happen at these various levels, but I don't think he ever argued that those levels were too high. I don't remember his actually saying that.

The Fallout Issue

Hughes: In approximately 1954, when this issue began to arise, this idea of a range of radiation exposure, range of risk, let's say, rather than a boundary, Linus Pauling somehow got hold of this argument

and, in Jones's view anyway, distorted Jones's argument, but Jones didn't say how. Do you know anything about that particular episode?

Grendon: Well, Pauling's work was not in the field of radiation. But after World War II, when nuclear weapons tests [and] fallout began to be a big issue, that's when Pauling and Sternglass and people like that entered into the radiation discussions. I don't believe it was because of their genuine scientific beliefs. They were opposed to the use of nuclear weapons, and they wanted to fight nuclear weapons in any way they could, and it really had nothing to do with the effects of radiation. They were just using that as a way to attack it. So Pauling began to write papers, or make public utterances at least, whether in speech or writing, about the effects of fallout. These were not balanced utterances. For example, Hardin and I both worked on some of these things where Pauling would say, for example, carbon 14 is a long-lived radioisotope. It has a half-life of something of the order of 5,000 years, and therefore an average life of the order of 8,000 years. And Pauling would say that if you had this much carbon 14 it would cause 2,000 children to die, or something like that, 20,000 or some other, bigger number, possibly.

Hardin and I wrote up a little response to the matter, which we never published, in which we said these 2,000 children, let's say, who would die according to this theoretical calculation, would die in a period when 3 billion people or 4 billion people or some other vast number would die. Where would you find them? If you lay out these 2,000 children in order to arouse an emotional response,

you must recognize that they lie among the billions of deaths here and therefore don't deserve this emotional response. Suppose that it's even true that they would [die], it's not a very big number among those who would die in the course of 8,000 years. He would compile all the deaths in that 8,000 years, you see. So you see this is not a fair way to look at it. There may be nobody on earth in 8,000 years, for all we know. We don't know what's going to happen to the earth in that many years. It is not an honest attempt to present a picture of what the risk is.

As I told the Women for Peace when they crowded into my office one day when I was the Atomic Energy Coordinator on the governor's staff.... They were demanding, on the basis of Pauling's remarks and other such remarks about the threat of fallout, that they must stop nuclear weapons tests because of this fallout threat, and the governor should speak out on it. I said, "Well, look, ladies. Suppose nuclear weapons tests help prevent nuclear war. Then we're talking about saving hundreds of millions of [lives] immediately, bang, at the time of war--hundreds of millions of people, as against the threat that in the course of time some people would die from cancer because of fallout. So you certainly wouldn't object to nuclear weapons tests then. But suppose, on the other hand, nuclear weapons tests increased the risk of nuclear war. Then you certainly should argue that [they] should be stopped, not because of fallout, but because they increase the risk that hundreds of millions of people will die suddenly in a nuclear war. What's it got to do with the threat that possibly, on

theoretical grounds, a few thousand people might die from fallout? It has no bearing.

Your important point is: Does it increase or decrease the risk of nuclear war? If you believe, as I think you do, that it increases the risk of nuclear war, then that's what you should argue. And I can't find fault with your argument. But if you believe, as I do, that it decreases the risk of nuclear war, then you certainly shouldn't worry about a little bit of fallout." But Pauling was trying to build up the fallout thing because he knew it appealed as an argument against nuclear weapons and nuclear war. I think that's misleading. Hardin Jones and I, as I say, drafted a paper, which we never published, that presented this viewpoint.

Hughes: Did [Edward Teller and Jones] ever confront each other directly?

Grendon: No. Pauling and Teller did, in a TV debate, but not Pauling and Jones. Pauling wrote to Hardin Jones, asking him to sign a petition with him. Hardin showed me the letter and said, "What do you think I ought to do?" and I said, "Well, you're not going to convince him. So you have a free choice. You can, if you wish, try to convince him, but I don't think you would. I think you'd waste your time. I think to ignore the letter is the best bet." So he ignored the letter.

Hughes: What was the [letter about]?

Grendon: It was asking him to sign a petition to join with him in this, and Hardin didn't do so because he recognized it wasn't sensible.

The Berkeley Student Revolt of the 1960s

Hughes: I was wondering if you could say something about Jones's personal relationship with John Gofman. What made me begin to think more about it was something that I read in a letter that Jones wrote to David Freifelder in 1966. He wrote that he kept Gofman productive over terrible difficulties. Do you know what he meant?

Grendon: Well, the Berkeley campus revolution began in 1964. Hardin Jones was the only faculty member, so far as I know, who spoke out about it immediately, who spoke out in defiance of it. I was one who also did, by the way, but I wasn't a faculty member.

Hardin told me of this experience of his on that first day. The students had demanded all sorts of things there and were creating a revolution. Not all students took part in it, of course. They marched down from the Greek Theater to Sproul Hall and said they were going to hold a meeting there, and Hardin demanded to be heard. They'd said to him that they'd give him a chance to [speak], but then he sat there and heard these revolutionary remarks and no response.

He asked the guy who was supposedly in charge of it to be heard, that they'd promised him an opportunity to be heard. They ignored him, and he just raised his voice louder and louder and said he demanded to be heard. And he did speak up on it--not very well received by the kind of crowd he had there.... I think they'd actually dared the faculty to come down to the campus--that was it--and he'd accepted the dare. He was the only one who did, with the promise that [the faculty would] be heard,

and then [the students] weren't letting him be heard. So after he raised his voice they had to let him be heard, and he spoke. From then on, he was outspoken against it and tried to lead faculty groups to resist this revolution.

The faculty of the University of California, and most faculties, are in general what is termed "liberal." That doesn't mean liberal in the old sense of being liberal, it means being anti-establishment. It means, in this case, accepting the revolution as a welcome change. When I heard [professors] say, "We learn more from you than you do from us," I thought, "Well, then you ought to start paying the students faculty salaries and drop the faculty." But they were being childish--"You're leading us!" Yes, [the students] were, but they were leading [the faculty] in the wrong direction.

Hardin spoke out against this, and I spoke out against it, and very few others did. John Gofman, on the other hand, was one who no doubt considered himself liberal, and he agreed with the revolutionary views. And so [Jones and Gofman] began to part company over their views about what was happening on campus, not over science. The difference in views over science came later when Gofman in his anti-establishment attitude was trying to attack nuclear energy and trying to adopt extreme positions there.

Hughes: Why did Hardin Jones feel that it was his burden to speak out?

Grendon: He felt that it was the whole faculty's burden and they weren't accepting it. I spoke to members of the Donner Lab faculty when an Academic Senate

meeting was going to take place, where they were going to vote on this, and tried to persuade them to go down there. They said, "I'm a scientist, you know. I don't get involved in this." I said, "But you are involved; they're going to speak in your name. They're going to say, 'The faculty says this.' And you're a member of the faculty." "Well, I'm a scientist. I don't want to get mixed up in it."

Well, you see, Hardin wasn't that kind of person. He felt that it was the duty of the faculty. He wasn't the only one. Although I was not a member of the faculty, I did participate in the meetings of groups of faculty members that he found were sympathetic, who shared the same view, and who wanted to do something about it--George Lenczowski, of the political science department, and David Louisell, now dead, of the law school, and some others.

Hughes: What about John Lawrence? Was he participating?

Grendon: John Lawrence was not actively participating. He did go to Senate meetings and vote in that [conservative] view. John is not the kind to try to do something overt about it, but he certainly agreed with that viewpoint.

Hughes: What effect did this controversy have upon Donner Lab?

Grendon: It was very divisive, very divisive. For example here, let me point out a personal instance. Because I wrote letters to the newspapers, which were published, criticizing this whole movement and

the people involved in it, a small group of faculty members began to attack me.

Hughes: You're speaking of Donner Lab?

Grendon: No, these were faculty members. One of them, Harry Rubin, who was with the Virus Laboratory, and a visiting professor of mathematics from Columbia, which happens to be my alma mater, jointly signed a letter which they sent to the papers, though no paper printed it because they recognized it was libelous. They also sent it to each of the Regents. [They said] in their letter to the papers, "You've recently published a letter from a Colonel Grendon"--I had not used the title Colonel in it--"who claims to be a biophysicist, though he has no scientific credentials. The only reason he is employed there is because of what Eisenhower termed the 'military-industrial complex'; this is the military involvement with the university." A whole bunch of tripe of this kind.

The reason I learned of it was that one of the Regents showed it to me. I said, "This is outrageous!" I asked some of my legal friends, including David Louisell, and he said, "Well, it's libel <u>per se</u>. There's no question it's libelous." So I decided I was going to bring suit. I'd been attacked many other times, but this was really vicious, to send it to the Regents and so on. This was just shortly before my mandatory retirement age, and when the Laboratory was trying to figure out a way to hire me under contract as a consultant.

Hughes: Was that coincidence, do you think?

Grendon: They didn't know of that. That was coincidence, the timing. David Louisell suggested a couple of lawyers, one in particular who was a Cal alumnus. I saw him and he said, well, there's no question it's a libel. So I wrote out for his use some of my credentials as to why I had a right to be there other than the "military-industrial complex." He filed a suit against Rubin for libel, and pointed out that [Rubin] had sent this letter not only to the papers, which didn't print it, but to the Regents, who might have control over whether or not I was engaged as a consultant.

Well, Rubin began to get very worried about this and had some of his faculty friends talk to some of the faculty in Donner Lab, who came to me to try to smooth this down and so on. They said that Rubin had a defective child for whom he was trying to set up a trust fund, and so on. I said, "Well, that's no excuse for what he did. He can't burden me with his personal problems when he's done this vicious thing." But after considering for a while I said, "Well, I'm not really trying to make money out of this, and I don't want to be hardhearted about it. If he will write a suitable letter of apology I will consider dropping the suit."

So he came up with a draft of a letter of apology. It was not a letter of apology; it was a letter of explanation why he did it but not a letter of apology. I said, "That just won't do. It's still not a letter of apology at all." So they made another try and finally came up with a letter. I said, "Well, it isn't what I would have demanded if I really wanted to be hard-boiled about

it, but it's sufficiently close that if he will send that to the papers I will drop the suit." Well, the papers didn't really report it properly. They reported that I dropped the suit, which wasn't satisfactory at all. But what do you do then? I had agreed [to drop the suit]. So all I could do is take his letter and write a little cover letter of my own and distribute it among faculty members that I knew were closely involved in it.

Hardin Jones knew Harry Rubin quite well. I didn't. As a matter of fact, as I pointed out, he had never even met me; he didn't know me at all. But Hardin knew him well and said, "He's really very, very apologetic about it. He feels very badly about having let himself be trapped into that, and he'd certainly like to erase it in some way." Oh, after a matter of a few years, when he would occasionally try to nod to me or say hello or something, I said, "Well, what the heck." So I would talk with him. But I couldn't find it possible to be friendly with him. I felt that what he had done was a mean, vicious thing to do.

While that was going on and a couple of people in Donner Lab were trying to get me to drop the suit, I decided that I ought to tell the people in Donner Lab about the particulars. They didn't really know what was going on. They just knew I was suing for libel. So I invited them to lunch at my expense.

Hughes: Who is "they"?

Grendon: The staff of Donner Laboratory, really--at least all the people I knew who were close to the situation. I think there were about 20 or so who

attended, and I explained the situation to them. Do you know, there were some of them who, in spite of what I just now described, felt that I oughtn't to have sued Rubin for libel. And there were others who felt that I was quite justified. So, when I'd intended this [lunch] to prevent a division of opinion, all I was able to do was observe there <u>was</u> a division of opinion, that these people were feeling strongly one way or the other. The same happened with the students who got arrested during the sit-in thing. There was division in the student body about what was right and what was wrong. Those who joined the revolutionary forces couldn't see why the others didn't join them, and those who were opposed to it couldn't see why these were revolting.

Hughes: What effect did these controversies have on the progress of scientific research?

Grendon: It made a tremendous difference. There was an uncountable number of hours lost, wasted on that kind of discussion and fight, heated argument, that should have been spent on scientific pursuits. The Laboratory lost an enormous amount of potential progress and productive effort in its quarrels over the political revolution on the campus.

Hughes: How did John Lawrence handle that? Did he do anything?

Grendon: No, John Lawrence didn't really run the Laboratory in that sense. He ran it in other senses, but...

Hughes: Well, even if he was apolitical, which I frankly don't quite believe--maybe he wasn't a

political activist, but I don't think he's apolitical.

Grendon: No, I think he's strongly conservative-minded, but he didn't do anything about it.

Hughes: Still, he could see that science was being affected in his establishment....

Grendon: He did nothing.

Hughes: What was the ultimate outcome?

Grendon: Well, it took several years for the thing to cool down. The ultimate outcome for the university, of course, was that the revolutionary forces largely gained their way. As a matter of fact, that was predicted by some of the faculty members not in the scientific area.

Hughes: Did Donner Lab lose anybody over this issue?

Grendon: Yes, I suppose it did. If you asked me to name names, no, I can't name names. At the moment at least, I can't think of anybody.

Hughes: It reminds me of the loyalty oath controversy in the late forties and fifties, where some prominent physicists left Berkeley just because they refused to sign the contract. I suppose that I could turn the question around the other way, too: What was the view of people considering Donner Laboratory as a place to work or study?

Grendon: That I don't know. I have more data on departments other than Donner Laboratory [and the] Division of

Medical Physics. In fact, [in] one of the letters that created that uproar where Rubin decided to libel me I pointed out what was happening in some other departments, where I had data as to the number of graduate students they would ordinarily recruit and the number who were coming in during that period, and what some of them said when queried about why they didn't come to Berkeley. But I don't have that kind of data on Donner Lab's graduate students. But it must have been the same thing there, because it was occurring in other departments, and there was political motivation throughout the university for all sorts of things.

For example, when a professor gives a final in biochemistry and asks, "What did you do during the Berkeley Free Speech Movement?" as one of his questions, this was nonsense, this was ridiculous. Or, I was planning to write a book about the Berkeley revolutionary movement, and I collected a lot of written material and a lot of tapes, and was threatened a number of times, physically attacked--only mildly--on at least one occasion. On another occasion they were threatening physical attack if I didn't give them my tapes to erase, which I allowed them to erase.

Hughes: "They" being students?

Grendon: This is students, yes. I attended public mass meetings of the students to get data for my intended work, and it was during those occasions that I got too well known by them and was always singled out.

I had in my files at that time, when I thought I'd write a book, a statement from one of the students who had taken a course in some scientific

area, but had been asked to write about the Free Speech Movement. The course may not have been in the scientific area. It may have been in English or something like that. I think it was in English, come to think of it. So he, according to his statement, wrote a paper favoring the Free Speech Movement, then wrote a paper opposing it. He was ambivalent about his own viewpoint. He decided the one he wrote against the Free Speech Movement was a better paper, so he submitted this paper, and he got a "D."

So he went to see the professor about this low grade. The professor, he said, swung around in his swivel chair and started talking about the Free Speech Movement. It was evident where his sympathies lay. The student recognized this and began to talk much more favorably about the FSM, and that he now understood much better. The professor swung his swivel chair the other way and said to his teaching assistant, "Give this student an 'A.'" The paper to go from a "D" to an "A"--that's nonsense, except in light of what the purpose was. The purpose was to win converts to that cause, and here he'd won a convert, so now he could get an "A" for his paper--which was really, presumably, a good paper, but just didn't have the right viewpoint.

Jones's Work on Drug Use and Abuse

Hughes: In reading through Hardin Jones's correspondence, which we have now in the Bancroft--did you realize that?

Grendon: Yes, I assisted in getting all that stuff together. Hardin and I had worked for many years together,

and when he died I'd stayed on and donated my time because of him, really. I said that I didn't see any point in staying on. We came to an agreement that I would stay on till the end of May--he died in the middle of February--and no later. I'd clean up what I could in that time and get the stuff organized. So I tried to get the stuff assembled and decide what could be used and what couldn't be used. When the Bancroft Library showed an interest in it, why, I helped to arrange the stuff that they took.

Hughes: Anyway, reading through the correspondence of the seventies, it seems to me that Dr. Jones became more and more involved in the political-social scene as opposed to the strictly scientific scene.

Grendon: Yes and no. When he was deeply involved in that political turmoil, one of the things that he noted was that the revolutionary movement and the drug movement were coincident, that the people involved in the one were involved in the other, that those who were pressing for revolutionary change were pressing for increased use of drugs. So he began to study the drug culture and behavior of the students involved in it. As a physiologist, he also made sure he was well informed in what drugs were being used and what their physiological action was. So he set up a course on drug use and abuse. I say "set up"--he proposed this course and it was accepted as a course to be given.

Hughes: In biophysics?

Grendon: Well, it was given under the biophysics program, I believe, but it was not limited to biophysics

graduate students. It was a course for anyone who wanted to take it. To his surprise, the very first time he gave it I think he had 300 students enrolled. Each time he gave it, and I think he gave it something like a dozen times, he had several hundred students enrolled. It proved to be a popular course. Some said at first that it was because it was a "Mickey Mouse" course, but it wasn't. He demanded work from them and had exams that really weeded out a number of them. But I think they were concerned with drug use because many were themselves involved in it.

So his scientific interest and his interest in social and political activities merged in that business. He recorded his lectures because he would not want to have a lecture written out. He'd have some very abbreviated notes and then start talking; to know what he had talked about, he taped each of his lectures. Helen Jones later took these tapes which she had transcribed and, with Hardin, worked up a book on drug use and abuse.

As a matter of fact, I worked up the index for that book at their request. I didn't get a chance to do my stuff on editing it--only belatedly when I was working up the index and reading the text. I found it needed editing badly and I gave Helen page after page of suggested corrections. Some of them were so urgent and important that even though it cost money to change the proof at that point, because it was already in proof, they had to change the things that were just plain wrong. But there were many other changes that should have been made in it, and if ever there were to be a second edition, she would have all these changes made.

Hughes: How was Jones's research on the drug problem received by the scientific community?

Grendon: He debated particularly with the pro-marijuana crowd. I forget the names of some of the people, but there were a number of people with scientific credentials, at least with medical credentials, who think he was totally wrong. Nowadays the evidence is becoming stronger that he really was right. But because they didn't want to believe it he entered into debates with these people on many occasions. But he was not alone. For example, when he testified before some congressional committee--I forget which one it was, but he testified for a couple of different congressional committees on the drug question--there were dozens on his side with scientific evidence, data on it.

His data were only of the nature of interview data, that is, case histories. In other words, he interviewed many hundreds of students who acknowledged drug use, and got a number of them to conduct an experiment that he found very successful. Of course a person who uses drugs and injures his mental capacities thereby has injured them enough so that he doesn't recognize that he's injured them. So they would contend that it hadn't bothered them.

He persuaded a number of them to try staying off of this drug for a month, off marijuana particularly. Other drugs were even more markedly troublesome, of course, but marijuana was the big bone of contention. Very few would defend heroin, very few would defend some of the other more drastically active drugs. But marijuana was the big issue. He'd say, "Well, stay off marijuana for a month, and then see." I met some of them who

said, "It was just unbelievable. After a month I realized I'd really been doped up. I realized that I was a different person." When they were under the influence of it, they would contend nothing had happened to them; they were perfectly competent in all respects. So the only way he could convince some of them is if he could get them to stay off it for a month; then they would realize that they really had changed.

Hughes: Was one of the goals of his course on drug use and abuse to convince students to [give up drug use]?

Grendon: I think that was what he started doing. That was one of the criticisms of his course, that it was intended to persuade drug users to give up that habit rather than limiting the course to a scientific study of the effects of drug use. In fact, the course did emphasize the risks to the users, but that could hardly be avoided.

Hughes: Well, given this background within the Donner Lab itself, the fact that this whole subject of the Free Speech Movement and the associated drug problem were very controversial, how was the course received, if it was given under the aegis of the Division of Biophysics and Medical Physics?

Grendon: It was known as Medical Physics number such-and-such, so it was under the division's program.

Hughes: I would have thought that would have been a red flag to those that were opposed to the issue.

Grendon: Well, it undoubtedly irritated them, but they had no basis for fighting it, in that there was no

question that it had scientific content and that Hardin Jones knew what he was talking about. There was, as I say, criticism that he was using it to urge [students] to change their ways, but I guess you could say that of dozens of other courses in other areas which were urging students to change their ways politically or otherwise. So I don't know that that really threatened the course at all.

Hughes: Well, is there anything more you care to say about that whole era?

Grendon: Let's see. I don't think I mentioned much about our work on carcinogenesis. I did say something about our studies leading to the conclusion that you had to consider latency as well as number of cases that might result from some carcinogenic agent. I don't recall anything that I would bring in at that point.

Research on Cancer Therapy Effectiveness

Hughes: At one stage, I think this was in the mid-fifties, Dr. Jones became worried about the statistical basis for the analysis of cancer therapy. He felt [that it] was on the wrong track, that there were inbuilt errors in the way that the statistics were built up.

Grendon: I know what you're talking of now. It was his review of the statistics on how effective surgical treatment of cancer or any treatment of cancer was. His analysis led him to conclude that they were kidding themselves. They were eliminating some of the data by the very methods they were using to

determine how effective cancer treatment was. He particularly focused in on several cancers, like breast cancer, where, according to his analysis, treatment on the average was not helpful. The test was, if the patient survived five years without symptoms, then you said that patient was cured. In the process of evaluating this, patients who died early in the study simply were eliminated from the treatment category, and therefore that biased the data as to whether treatment was useful or wasn't useful. I don't recall the detailed analysis, but I do know that his conclusion was that patients who were diagnosed as having cancer were just as well off gambling on not receiving treatment as receiving treatment. You spoke of radiation treatment, but it wasn't just radiation treatment, it was any treatment, surgery or...

Hughes: It was all of the types.

Grendon: That's right, yes. Untreated cancer cases had as good a prospect of longevity as treated cases.

Hughes: How was such a revolutionary idea received?

Grendon: Oh, the medical profession was very indignant about it, and contended whenever they could that he was totally wrong. Even he and John Lawrence didn't agree on this at first, John Lawrence being a physician, you see.

Hughes: John Lawrence also having been engaged in neutron therapy [for cancer], too.

Grendon: Yes, and in therapy of various kinds. John Lawrence worked with radioactive phosphorus on

polycythemia vera, for example, which was one of the early medical uses of radioactive materials. But the medical profession was very reluctant; in fact I don't think it did accept Jones's hypothesis.

Hughes: What was their basis for discounting it?

Grendon: Well, most of them, of course, who protested really weren't capable of doing a statistical analysis themselves. They merely were sure that they were helping [their cancer patients].

Just a few months ago a physician from Germany, who had come to a similar conclusion and who had written some material and was writing another book, tried to get in touch with Hardin Jones. Learning of his death and learning of my existence, he wanted to talk with me. I said, well, I can't really help you very much there because that was not one of the aspects of his work that I got involved in. He had done that before I even got to know him. But he was insistent that he wanted to talk with me, and I said it's a long way to come up from San Francisco, but if you insist, you may. He came up and we talked for a little while through an interpreter. My German is not that good. I can get by if I must, but not in a scientific discussion. I'm sure he didn't get anything from it. He left a couple of his books with me, which deal with the same thing--that there's excessive surgery for cancer, in his view.

Hughes: He'd arrived at those opinions independently?

Grendon: Independently, apparently, and learning of Hardin Jones's work on it wanted to talk with him about

his basis for concluding that. In fact he asked for the paper and I said, well, unfortunately, when I was looking over these papers, there were pages of miscellaneous data that I couldn't interpret and I simply said I don't see that they're interpretable, and so I disposed of them. And he said, couldn't he get them and refer them to a statistician? And I said no statistician would know what these numbers meant. They're just pages of calculations that are not identified.

Hardin unfortunately left a lot of his work unfinished over the years--not merely at the end of his life. He'd get a lot of this material and work it up on scraps of paper and then leave it as that and never report it formally. That was too bad, because much could have been gained from it.

Jones as a Scientist and as a Person

Hughes: Well, talk a little bit, if you would, about what he was like as a scientist and also as a person. Can you say something about how he approached his research, how he developed his ideas?

Grendon: Well, of course what I mentioned before was his scientific intuition. He would get a hunch that something or other produced this result, and then would try to find data that either supported or disproved that contention. I think most scientists begin with, tend to be swayed by, their convictions, to look for stuff that supports their views. But I don't think that he was more so than most scientists in that. I think he did have a tendency to look for material that supported a view that he had already arrived at. But, as I say, I

think that's a common characteristic that's hard to avoid.

Hughes: Are you aware of instances where the data did not support his hypothesis?

Grendon: No, I don't think he would shut his eyes to anything that was against it. For example, I spoke of that paper we wrote on latency, and we acknowledged in it that there were some carcinogens that didn't seem to behave that way. We mentioned them in our paper and mentioned some other theories about what might be happening there--that urethane, for example, as a carcinogen didn't seem to behave in this way that we described. We recognized too that although we said that it was ordinarily an inverse of the cube root of the dose, that we found instances where it was really a much higher power, even as high as the sixth root. But still, that it varied inversely with some power of the dose seemed to us important. We could offer a hypothesis why the cube root would be a natural figure to arrive at, though it didn't always occur that way, because there obviously were other factors at work that we didn't know of. So I don't think he'd close his eyes to data on the other side, though he certainly was eager to find supportive data--as any scientist is likely to do, once you've reached a point of view about what it is that you think is operative.

I spoke also of his skill as a laboratory worker, not only skill in manual dexterity and things of this kind, but inventive skill, inventive in ways to get around some difficulty and find a way to test the thing you're trying to test. One of the things that he and Ernest Dobson worked at

earlier had a scientific application that still is used. They found a way to produce a radioactive colloid which would be picked up by the liver and therefore be usable as a test of liver function. This was rather tricky to make, but they found a way to make it, and were so successful in it that others weren't able to copy them very well. For a long time, Donner Laboratory, in spite of its efforts not to be merely a production enterprise, had to supply this chromic phosphate, I guess it was, to physicians and other investigators because nobody else was able to make it. They didn't really want to be a factory, but they felt obligated to supply it until somebody else could find out how to do it.

Hughes: Why was the use of chromic phosphate dropped?

Grendon: Well, it wasn't dropped, but others did finally learn how to produce it. They only produced it as long as necessary, to satisfy demand when nobody else could.

Hughes: Do you have an explanation for Jones's seeming tendency to carry research just so far and then to go on to other things? I mean the fact that he left so much unwritten-up.

Grendon: Most scientists are reluctant writers. Most of them are also very bad writers. If you've ever studied scientific writing, you'll find that it's very poor.

Hughes: I think I have a very good idea.

Grendon: I think that was just it, that he didn't like the task of writing this stuff, and so he'd put it aside for a time. And having put it aside for a time, it would stay aside. He was not a good writer. I edited almost everything he wrote after I became his collaborator, because I'm pretty good at writing, and he was not.

Hughes: In what sense not good?

Grendon: [He] couldn't state what he meant clearly, unambiguously. That happens to be one of my better characteristics, my better qualifications; I've been told by many people who are good judges. At the time, for example, of my contests with the revolutionary left, they admitted that they admired my writing, that it was so clear and to the point, and they wished I was on their side. Rubin said that, through an intermediary, who said, "He wishes you were on their side because you write so well." Well, I do state the case clearly, and Hardin Jones often couldn't.

His spoken remarks [were] even worse, because he would say things he didn't mean. If I was present when he did it, I'd raise my hand and say, "Don't you mean this?"--which would be quite the opposite--and he'd say, "Oh, didn't I say that?" When I'd help him by editing a transcript I'd say, well, good grief, he said this? Obviously he doesn't mean it. And when I'd correct it he'd appreciate the correction. But even when he'd write, he often would be very careless with it, and he didn't like doing it, couldn't state his case clearly.

I mentioned earlier that we complemented each other with our talents. I'm precise. When it came to calculation he'd always turn to me and ask me to do the calculations because he would be rather careless with [them], and I'd find an error in [them].

Hughes: Are you referring to statistical calculation?

Grendon: Statistical calculation work, anything involving numbers.

Hughes: And what was he contributing to the partnership?

Grendon: Well, he was contributing ideas--not that I didn't, but he was very good with these scientific hunches, let's say. He'd come up with a hunch, and we'd work on that, and it would turn out that the hunch was good.

Hughes: What was his relationship with students, both undergraduate and graduate? Did he have graduate students?

Grendon: Oh yes, for many years he had large numbers of them. In later years he tried to lighten his load in that respect, but he still had some. They admired him. It's a strange thing, you see. As I mentioned, he'd make mistakes in what he'd say or write. Now, when I was his student, in that body of military students, they all thought he was wonderful. I recognized that he was saying some things that he couldn't have meant, even then. But other people aren't that careful, they are not that careful listeners, they're not that careful readers. And so students rarely would find any

difficulty in what he'd say, though they should have. No, he was admired by students. Oh, when he gave his course on drug use and abuse, there he wasn't universally admired, because many of the people who took the course were drug users, who came there with a chip on their shoulder, wanting to heckle him, wanting to bother him. It didn't bother him. He managed to deal with hecklers quite well.

Hughes: What was his role as a supervisor of graduate students? How much did he participate in a graduate student's research?

Grendon: In his work with graduate students, often he would help them over key spots where they were stuck, with this same laboratory skill of his. I mean he would suggest ways for them to get around the difficulty. As I say, later on he tried to ease his load on that. Well, of course, towards the end, he knew in the last year or so that he was really very tired.

He and his wife made a trip to Australia and New Zealand, I think--I know Australia, anyway--at the invitation of the Australian government, to help them with their problem of drug abuse. He talked many places and did a lot of traveling. That was about a year before he died. [He] came back from that trip very tired, recognizably tired, and I wouldn't doubt that that's what started his medical problems that led eventually to his death. There's no way to prove that, of course. Those of us who were closely asociated with him recognized signs of obvious fatigue, and he asked to be relieved of much of his load, and so on.

Throughout his association with the Laboratory, he and Dr. Born were labeled assistant directors of the Laboratory under John Lawrence, and Hardin really handled all the research programs. He directed them. John Lawrence really didn't direct any except the ones he was intimately associated with.

Hughes: What do you mean when you say "direct"? What kind of things would he be doing?

Grendon: He guided the programs into areas that seemed fruitful, and advised students, even though they weren't all his graduate students. He would even advise their advisors as to ways to try to keep the program of the Laboratory working in promising areas.

Hughes: How was he given that role?

Grendon: Well, John Lawrence passed it on to him.

Hughes: Did John Lawrence do that because he recognized Jones's talent for that?

Grendon: Yes, and he didn't like doing that. Jim Born would deal with personnel and with the medical area, and Hardin Jones dealt with everything else.

Hughes: And what was John Lawrence doing?

Grendon: Dealing with his own research, and he had a few graduate students working with him. But, you see, Donner Laboratory conducted programs in both biophysics and medical physics. Physicians taking the same kind of work, doing the same sort of

thing, were called students of medical physics because they were physicians. John Lawrence would work much more closely with them, because he has a bit of rapport with these other physicians. So he had some of them under his wing directly, as graduate students there. But as for the work of the Laboratory as a whole, he had to represent the Laboratory of course in terms of budgeting and such, but I don't think he tried very much to get himself involved with the ordinary operation of the Laboratory.

Hughes: And was this pretty much true throughout Dr. Jones's tenure as an assistant director?

Grendon: I think that's true from the beginning.

Hughes: Well, the way you describe it, it sounds to me as though Hardin Jones had a bigger piece of the pie than would normally be assumed with his title.

Grendon: That's right. I think it was a much bigger piece of the pie, really.

Hughes: I believe his tenure in that position ended in 1976.

Grendon: When John Lawrence retired, not forcibly retired, but retired prematurely in order to become a Regent, Ed Alpen became the director. Alpen's views about the Laboratory were quite difference from John Lawrence's, so he started out by reorganizing the whole thing. He had no assistant directors at all. He abolished the post of assistant director. But he kept Hardin Jones on as

a consultant on--I forget what the term was that was used in the organization chart.

I don't know whether this ought to go on the record, but when I first came to the Laboratory, I asked one of the men there, "I don't quite get the organizational structure of this place. Does it have an organizational chart?" He said, "Well, it doesn't really, but I can draw one for you." And he wrote in a little block John Lawrence's initials, JHL, and a little stem down to a series of blocks that went around a circle here. "Who are all these people?" He said, "That's about the organizational structure of the Laboratory."

Hughes: You mentioned that Alpen's views differed from John Lawrence's....

Grendon: Well, he ran the Laboratory. Ed Alpen wants to run the Laboratory. He wants to be in charge. John Lawrence was content to be only nominally in charge.

Hughes: He was delegating responsibility.

Grendon: Yes. Alpen, as I say, abolished the post of assistant director. He gave Hardin Jones the post of an advisor to the director, a consultant.

Hughes: What did that entail?

Grendon: Well, it didn't really involve as much change as it might seem, because it still meant that he got to discuss what the programs of the Laboratory should be, where they might look for funds, and so on. It takes a lot of time for somebody to try and raise funds for the Laboratory, for any such

establishment. You've got to make applications to foundations and national organizations like the National Institutes of Health or other agencies for any project that you need there.

The Laboratory for years was funded exclusively by the AEC, then when the AEC was abolished, first the Energy and Research Development Administration came in as an intermediate thing, and then shortly after, the Department of Energy. You still have to justify each project as falling within their zone of interest. So you have to change projects often to make them fall within that zone of interest, and that takes a lot of time and activity. Hardin Jones was involved in a good deal of that, trying to make the programs fit the funding available. This has been one of the criticisms about federal funding of university programs over the whole country--that you tend to do what the fund provider wants you to do rather than what your scientific interests would lead you into.

Hughes: I would surmise that Jones himself ran into some of these difficulties, did he not? I'm thinking in particular of the AEC days, when research at Donner Lab was supposed to be restricted to the use of radioisotopes and radiation.

Grendon: Well, something to do with the radiation question.

Hughes: I mean, for example, the lipoprotein question. Did they ever run into problems because the association there was rather vague with the so-called mission of the Lab?

Grendon: Since that was before my time really, I couldn't answer that, but they seem not to have [had much trouble] in the early days. In the early days of the Laboratory, apparently the monitoring of the use of funds was much looser than it became later on. These were the days when money was freely available and so there was no close monitoring of what was done with it. But when budgets began to be tighter, and the AEC, through its Division of Biology and Medicine, for example, would find that it got less money this year than it had last year and everybody had to take a cut somewhere, then it would begin to question more: Why are you doing this? Why are you doing that?

But at the time they were doing the lipoprotein work, money was fairly free, and so they didn't impose very strict guidelines on what projects you spent it on. If the scientists thought they were doing something useful, they'd let them go ahead and do it. I don't suppose that lipoprotein project could have got started in later years.

Jones's Relationship with John Lawrence

Hughes: Can you say anything about the personal or scientific relationship between Dr. Jones and Dr. Lawrence?

Grendon: Well, their personal relationship was very close. They shared many viewpoints, other than the scientific one, and so they found themselves congenial. In the scientific area, because Hardin Jones had such a broad span of knowledge and had spent a lot of effort in guiding research throughout the Laboratory, he worked closely with

John Lawrence on John Lawrence's areas of interest and research. That is, he would consult with him. They had some papers in which they were coauthors, I'm sure; I don't remember which ones. Well, you have a bibliography for Hardin Jones. You could easily see in it whether Lawrence's name appears. I'm sure it does. For example, I think John Lawrence's name appears as coauthor on some of the lipoprotein work.

Hughes: Yes. You're right.

Grendon: Because as a physician he would be interested in that, he would get involved in discussion of it. You must remember that often these coauthorships really represent the fact that these people would talk about the project together. They wouldn't all be doing something specific about it, but they would talk about it. And in the course of these discussions, one remark would lead to another, which would lead somebody to say, well, what about this? and they'd do some work that related to it. So you'd say, well, his conversation's in it, his discussions entitle him to some credit in authorship. So coauthorship in many of these doesn't mean a whole lot. You see seven or eight names as coauthors on a paper and you know they can't all have done anything very vigorous about it. But they participated in the discussions that led to the results.

Hughes: Can you think of any examples of the sorts of subject matter upon which John Lawrence would turn to Hardin Jones for advice?

Grendon: Well, as far as running the Laboratory, he'd turn to him on almost anything there, because John Lawrence didn't want to be the one to do it, really, and he was passing it to Hardin.

Hughes: In terms of Lawrence's own research?

Grendon: Well, in terms of his own research, [Lawrence] would certainly talk with [Jones] about anything where he was uncertain, let's say, about what might be producing a certain effect or what course to follow. Now, I wasn't in on such discussions but I know from his general attitude that undoubtedly he would have done that, because that would be the natural thing to do--talk to another person involved in that kind of work. In terms of calculating radiation effects, Hardin would be much better than John Lawrence on it. John Lawrence really wasn't involved in that kind of thing. This is an imaginary conversation: He might say, "What do you think, Hardin? Would this be dangerous?" The reason I say that is he threw that question to me one time in a different context.

Debate with Edward Teller

The American College of Physicians, I guess, were holding their annual meeting in San Francisco, and Donner Laboratory was putting on a closed-circuit program for them. I was at that time up in Sacramento rather than at Donner Laboratory, and John Lawrence called me up and told me of this program they were putting on. As a close to it, since they'd been talking about use of radioactive iodine in thyroid work, he wanted to have Edward Teller and me on the program and throw us the

question, "Is this use of radioactive iodine too dangerous to be continued?" and have us discuss it unrehearsed. I said you know Edward Teller and I would get into an argument over that. "Well, would you be willing to do it?" So I agreed I would. It was to be unrehearsed, but nevertheless I felt we ought to get together on what areas we were going to talk about.

So I called Teller and he said, "Oh, I was going to call you." I said I thought we ought to at least agree on what areas we'd touch on, even though we [didn't] want to rehearse the program in advance. And I said I'd given it some thought, and thought we ought to deal with this topic. "No, but we should deal with this." I said, "Well, okay," and I said, "I also thought we might deal with this." "No, no, but we should deal with that," and I said, "Well, okay." So on each point I was being overruled. I was accepting Teller's suggestions. We came to one area there about some of the risk factors, and I said, "Well, you know we don't agree on that. We don't really have to introduce that. We'll have enough material without that, so perhaps we should stay away from that topic." So he agreed we would.

We were on the program, and John Lawrence had thrown this questions at us and we were discussing it. And lo and behold, Teller got into this area and made his remarks. I thought, well, we'd said we wouldn't, but since he has... I raised some objection to it and presented a different viewpoint. He got more heated; the program closed on this note where he was very irritated.

At that time I was the Atomic Energy Coordinator of the State [of California] and I had an advisory committee on which Teller was one of

the members. Well, he got up from his chair and said, "I guess I might as well resign from your advisory committee." I said, "No, Edward, don't do that at all. I value your opinion." "Well, you saw the way we ended." I said I didn't think that was very bad, and I turned to John Lawrence and said, "Did you think that gave a bad impression?" He said, "No, I thought it was very interesting the way it turned out."

I'd had one of my friends taping the thing there, and because of this argument, I said to [Teller], "Let me use that tape and ask Hardin Jones and John Lawrence to listen to it and then tell me whether they thought this gave a bad impression." They said, "We thought it was fine." I said, "Well, Edward Teller doesn't think it's fine. I didn't agree with him, and you're supposed to agree with him." So he didn't get off my committee, and just the other day we were at a symposium together and we were very friendly.

Hughes: May I ask what sides of the fence you were both on?

Grendon: Teller is defending nuclear energy and doesn't want to be realistic about it. At one point in his earlier career he quoted an experiment that was done by L.D. Carlson up at Washington at that time, and an experiment by Egon Lorenz, in which animals irradiated at the lowest level outlived the controls on the average. And he said, "So you see, maybe a little bit of radiation is good for you." Well, I won't concede that.

I have thought of a theory since that point, that I was planning to investigate at a point where I thought I would stay on the payroll [of Donner Laboratory], and then I didn't stay on the payroll

so I never did investigate it. But this theory... I said there is a possibility that a little radiation is indeed good for animals because mammalian organisms, at least, have learned--their systems have learned to do certain things under certain conditions. When they're invaded by an infective organism, a pathological organism, they learn to muster their reserves of white cells to fight off this infection and so on. They've learned to counter these things when they are assailed, when they meet an insult.

Now, suppose this low level of radiation is recognized by them as an insult to the system and so they muster their reserves to fight an infection. Animals that would have died of pneumonia or nephritis or some other infectious disease may have mustered enough resistance under the threat of radiation to be able to fight these off. And so if you consider their average life span, you might find that the low-level radiation group really does better than the control because of their resistance to these diseases, possibly.

I devised an experiment to test this, as a matter of fact. But I said, in the case of man, that doesn't help him. He has other means of fighting these off. He has antibiotics that take care of all these infectious diseases. We can wipe them out. But if this low-level radiation tends in the long run to produce cancers among men, that makes a difference. You wouldn't find it in the animal group because of the overwhelming improvement against infectious diseases. So I was going to treat a group of animals with antibiotics, routinely, and subject them to a low level of radiation, and treat a group of controls with

antibiotics routinely, and see what happened between those two groups. If then you found the irradiated group lived long enough to develop cancers and the other ones didn't, that would be our answer as to whether this was harmful or not.

But to say it's beneficial to man? No. To animals? Maybe yes. But not certainly, because both those two experiments had problems other than simply that, and so you couldn't really draw any sound inference from [them] in any case. At best, you might be able to draw the inference that really low-level radiation does help animals in that respect. My experiment never did materialize, because, as I say, my term of service reached an end on account of my being too old to be of any use.

The Institute of Human Development

Hughes: I saw a reference to an Institute of Human Development.

Grendon: Yes, the psychology department and Hardin Jones, from Medical Physics, got involved in a long-term experiment years ago, in the 1930s. They wanted to see whether physiological and psychological factors that you could study in children and adolescents could be used in any predictive way as to what would happen to them in later years. So they took a number of Berkeley school children and studied them, with the permission of their parents and the school. [The psychology department] studied psychological factors. A Harold Jones, long dead, was their leader there. So two Joneses were involved who weren't related in any way except by name. Harold Jones and his colleagues worked on

the psychological tests that they devised. Hardin Jones devised a set of physiological tests which he and Margaret White applied to these students and recorded in the thirties, when these students were ten or twelve years old. Then, at ten-year intervals, they tried to call these same people back in and retest them in similar ways.

By the time I arrived on the scene, these people were in their mid-thirties, I guess. They were called in one more time, and I took an interest in some of that--in fact, enough of an interest that Hardin wanted me to use that as the basis of a Ph.D. thesis. But then this other thing [atomic energy coordinator] came along shortly after and so I never did. From this set of data [Hardin] was trying to find out the predictive vaue of any characteristics that they could find in the earlier years--prediction as to their state of health. The psychologists were trying to find a predictive factor as to their state of mental health, and also as to interrelations between them; mental characteristics may affect physical well-being and vice versa.

So, [about] the time that you mentioned there, [Hardin] and Harold Jones talked about setting up an institute of human development that would deal with these things they'd been measuring over the years and study them jointly. Hardin was asked to be director of this institute and he asked me if I would work with him on that. I said I'd at least be interested in what might develop from it. So the question became one of trying to get through the mechanisms of setting up an institute: the funding and the approval of the faculty and the academic senate, or whatever it takes, and various things of that sort. About that time it got

involved in academic politics, I think. I wasn't closely involved in it, but I think that's what it was. [Hardin] was named for a time director of the institute, I believe.

Hughes: Yes, he was. I think only for a year.

Grendon: That's right. Then the psychologists decided that they ought to take it over. There was a lot of inside politicking and so on, and he was replaced by someone from psychology.

Hughes: How was his time divided at that stage? Was this taking up quite a bit of his time from his normal work on medical physics problems?

Grendon: Well, this was his normal work, to study these physiological aspects, so he wasn't being dragged away from it. When he and I were both working on these physiological characteristics, that was a legitimate work of Donner Laboratory. So far as the other aspects of it--the administrative, the budgeting question, the working out a table of organization and all that--well, if you are trying to set up an institute, you've got to do that. So, yes, it took away from scientific activity, but not a whole lot. I helped him write up some prospectuses for what the institute would do and what it would require to do it, in terms of personnel and funds. But it didn't take a whole lot of time.

Hughes: When he stepped down from the directorship, what did that do to his participation in the institute?

Grendon: I don't know what the institute's history has been since then. I think it did persist. I'm not sure.

Hughes: I looked it up in the telephone directory and I couldn't find it.

Grendon: Well, I said I thought it persisted, but I wasn't sure. I think what actually happened is that the psychologists who tried to take it over and succeeded in taking it over at the time weren't able to get faculty approval and funding for it, enough to make it go. Although I said I thought it persisted, I wasn't even sure of that and I guess they just didn't make a go of it. [Hardin] felt that he'd been pushed out of it and he didn't really care that much. We were content to work with what we had on the physiology....

Hughes: What was the funding of the institute?

Grendon: I don't know. I don't think they got what they asked for, by a long shot. I don't think they got enough to make it a go. They did for a time have a space in a building there, one of the separate buildings on campus, near the law school. I didn't really follow it closely. To me it was a secondary issue. I was just trying to help them a little bit.

Donner Laboratory as an Institution

Hughes: Do you know anything about how new people were brought into Donner Laboratory?

Grendon: There was no dearth of applicants. Donner Laboratory always had scores of applicants, more than they could accommodate. At one point John Lawrence had a map of the world, that is, a plane projection, with pins showing areas of the world from which we at that moment had graduate students or visiting scientists who were doing research. People from all countries of the world wanted to be here, as well as graduate students from the United States. So there was never a shortage of applicants. It was just a question of trying to pick the best ones for the spaces available, and ones whose interests coincided with what the Lab wanted to be interested in.

Hughes: Who made those decisions?

Grendon: As much as [anyone], Hardin. It would be talked of between John Lawrence and Hardin Jones, and even Jim Born was entering into some of these discussions.

Hughes: What about Cornelius Tobias? Was he entering into much of this?

Grendon: Not a whole lot. He was very active in his scientific work, but I don't think he bothered too much with anything beyond that. That's my opinion. I don't know that it's so.

Hughes: What was James Born's relationship with Hardin Jones?

Grendon: Oh, they got along fine. John Lawrence and Jim Born and Hardin Jones shared the same sort of social and political views, I would say, and were

very congenial with each other. There's no problem there.

Hughes: Do you think that had any effect on the operations of the Lab? Were there larger repercussions?

Grendon: No, I don't think it did until possibly the time of the Berkeley revolution. Then it made a difference that they were able to stay together, because they shared similar viewpoints about the revolution also. Therefore, at least at that level of the director and the assistant directors, there was no break-up, which there might have been if they differed about what was going on on campus.

Hughes: The great majority of Donner Laboratory personnel has been educated in Donner Laboratory. What effect, if any, do you think that this has had on the Lab?

Grendon: This inbreeding, you mean?

Hughes: Yes, inbreeding is the way it's usually expressed.

Grendon: It hadn't been entirely that, but it had been, as you say, largely that. I don't think it's been adverse. When I was a student there, I had as fellow students some of these people who were scientific staff of Donner Laboratory when I came back later. I don't think it's been an adverse factor because it isn't like inbreeding in an animal species. People are individuals, no matter where they got their training. If they have research capabilities, they're using their minds. Under what auspices they took their training isn't that significant. You don't get your thoughts

colored by it. You learn certain ways of doing things, I suppose, but you change them as you find it necessary to change them. So I don't think there was any adverse effect from that. It is true, though, that a large part of the staff were educated at Berkeley and in Donner Laboratory.

Hughes: When you have this research in groups, what happens to communication among the groups?

Grendon: Well, there wouldn't be very much direct communication. But Donner Lab wasn't that compartmentalized. For example, for years it had Monday seminars at which people from other institutions or people from Donner Lab would present what they were working on, and everybody would enter into discussion of it. When somebody from Donner Lab, let us say in the lipoprotein group, would present a paper on what his group was doing, all these other people present could and did enter in discussion of their results and offer criticism sometimes, but mostly pointed inquiry. So there wasn't lack of communication in that sense, but as a group they wouldn't come over and ask them, "What do you think we ought to be doing here?"

Hughes: Was John Lawrence himself doing that kind of thing ever?

Grendon: He would be in on these seminar sessions pretty regularly. At least during my period of observation there, I'd say he was at much more than half of the seminars.

Hughes: Was he the sort of director that would pop into a lab and say, "Hey, Joe, what are you up to?"

Grendon: No, no. That's not his style.

Hughes: The picture that I'm getting is that if you have to pick one individual who was aware of the science that was going on in Donner Lab, you'd have to point to Hardin Jones.

Grendon: That's right. I don't think there was any question. I think he was much more conversant with and influential in directing the research program of Donner Lab than any other one individual.

Hughes: What about obtaining funds?

Grendon: He was quite a promoter of funds, too. It took time that he shouldn't have had to spend on it, but he would be very successful in doing it.

Hughes: How did he do that?

Grendon: Well, if you apply for funds from some agency, you have to meet their requirements as to how you present your case, whether it's just a letter or whether you fill out some kind of forms, and so on. With the AEC, you had forms. There's a Form 189--it's a famous number that every project has to prepare, and they end up with a big fat book of Form 189s for the various projects going on. If then the people who were considering that project raised some objection to it, you had to defend it. It was in this kind of defense of a project that [Hardin] would be very able.

Hughes: Would he also know people to direct applications to?

Grendon: Oh, sure. In the course of years you get to know all those people quite well. You call them by their first names, and they call you by your first name. You pick up a phone and talk to them and say, "Well, look, Nat, now on this project we're really aiming right at this that is useful to your purposes, and there's nobody else doing it in any other laboratory," and this and that and the other thing.

Hughes: Did he ever object to you or anybody about the amount of time that he had to spend on this kind of matter?

Grendon: Oh, yes. He felt it was very regrettable that you have to spend that much time. But if you have to do it, you have to do it.

Hughes: What would he prefer to have been doing?

Grendon: He'd prefer to be working on the scientific questions. Basically his interests were in science, not in management. But since he got involved in management, he had to do what it takes to get funds and handle the administrative issues involved.

Hughes: One final question, unless you have anything to add after that: As a person who has seen Donner Lab both from the inside and from the outside, how would you assess the Laboratory's standing on a nationwide or even on an international scale?

Grendon: Well, for many years it certainly was highly ranked. That's the reason it had so many people wanting to come to Donner Lab from all over the world. Its name is still very potent in that. I think that the interruption at the time of the Berkeley revolution hurt it. I also think that the repreated cutting of funds hurt it. I think that the change in emphasis, from where you could use the funds that were allocated to you in ways that weren't really quite in line with the funding organization's needs, to now, where each project must be goal-oriented in the needs of the funding agency--I think that has impaired its scientific status. So although it still has a very fine scientific reputation, it really is not as good scientifically as it once was, in my own view. Other national laboratories, Brookhaven, Argonne, and so on, I'm sure are faced with the same thing. So I wouldn't attempt to rank them in any sense. But I wouldn't doubt that they all have been lessened by the budgetary restrictions and by the specific goal-orientation required for projects.

Hughes: Do you think those cutbacks were more severe in the case of Donner Lab than Argonne or any of the others?

Grendon: I don't know. I don't really think so. I think they all were being cut back. Congress was cutting the funds for the AEC and then later for the Department of Energy. That was the factor, particularly in the AEC period, where the cutting back of funds led to a cutting of all the laboratories that were supported by them.

Hughes: Is there anything more you'd care to add?

Grendon: I don't think of anything, no.

Hughes: Well, I thank you.

[End of Interview]

Index - Alexander Grendon

Academic Senate (UCB), 44
Alpen, Edward, 67-68
American College of Physicians, 72
Argonne National Laboratory, 26, 85
Atomic Energy Commission (AEC), 26, 35, 36, 69, 70, 83, 85

Bancroft Library, 52, 53
Bond, Victor, 23, 25
Born, James, 66, 80-81
Brookhaven National Laboratory, 23, 85
Brown, Edmund G., Sr., 3-5
Bureau of Radiological Health (California), 26

California Department of Health, 27
Carlson, L. D., 74
Chaikoff, I. L., 10
Committee for Consideration of Human Application of Radioactive Materials (UCB), 34-36
Congressional Record, 23
Council of State Governments, 3
Cowell Hospital (UCB), 20

Dobbin, E. Virginia, 18
Dobson, Ernest, 61
Donner Foundation, 11

Free Speech Movement, 56
Freifelder, David, 43

Gofman, Helen, 18
Gofman, John, 13-20, 21-26, 28, 29-31, 38, 43-44
Gottschalk, Alexander, 26

Health Physics Society, 7, 26
Humboldt Bay, California, 27

Institute of Human Development (UCB), 76-79
International Commission on Radiological Protection (ICRP), 37

Jones, Hardin, 1-11, 13-14, 16, 20, 21, 29-30, 32, 34, 35, 37-38, 39, 40, 42, 43-45, 48, 52-72 passim, 76-79, 80-81, 83-84
Jones, Harold, 76-77
Jones, Helen, 18, 54

Lawrence Berkeley Laboratory (U.C. Radiation Laboratory), 6
Lawrence, Ernest O., 6, 11, 20
Lawrence, John H., 5, 10, 11, 45, 49-50, 58-59, 66-68, 70-72, 72-74, 80-81, 82-83
Lenczowski, George, 45
Lindgren, Frank, 13, 19
Lorenz, Egon, 74
Louisell, David, 45, 47
Lyon, Lenore, 18
Lyon, Thomas, 18

National Bureau of Standards (NBS), 39
National Council on Radiation Protections and Measurements (NCRP), 25, 36-39

Naval Postgraduate School (U.S. Navy), 1
Neyman, Jerzy, 28

Pauling, Linus, 39-40, 41, 42
Proceedings of the Health Physics Society, 7
Rubin, Harry, 46-49, 51, 63
Saenger, Eugene, 27
Sagan, Leonard, 25
Siri, William, 13
Sternglass, Ernest, 22, 23, 26-29, 30, 40

Tamplin, Arthur, 17, 24
Teller, Edward, 42, 72-74
Tobias, Cornelius, 1, 2, 15, 80

U.S. Army Air Corps, 11
U.S. Congress Joint Committee on Atomic Energy, 9, 23
U.S. Department of Defense, 1
U.S. Department of Energy, 69, 85
U.S. Energy and Research Development Administration, 69
U.S. Navy, 1, 11
University of California Regents, 46, 47

White, Margaret, 7, 77
Women for Peace, 41

Young, Clara-Beth, 18

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